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PERMANENT ELEMENTS IN THE FLUX OF PRESENT-DAY PHYSICS[1]

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MANY of us could, I believe, confess to a feeling of breathlessness at the rapid changes of our present physical progress, and some of us might even, in a moment of candor, admit a little resentment at our shortness of breath. Let us discuss together what we may perhaps best do to recover our poise.

The changing situation which is responsible for our discomfort is complex. First and foremost there is our changing experimental knowledge, reaching over the entire range from the infinitely small to the infinitely large. The upsetting feature here is not so much that we have discovered an enormous array of new facts, which in themselves are difficult enough to keep pace with, as that these facts have proved in many cases to be irreconcilable with our previous expectations of what was possible, so that we have been forced to change our entire conceptual attitude. These conceptual changes have in many cases been associated with mathematical theories, which are being continually formulated at an ever-accelerating tempo and in a complexity and abstractness increasingly formidable. Some of the more important landmarks in this progression are: The electromagnetic theory of light, the special theory of relativity, the general theory of relativity, the quantum theory of Bohr, the matrix calculus of Heisenberg, the wave mechanics of Schrödinger, the transformation theory of Dirac and Jordan, the group theory of Weyl and now the double quantization theory of Jordan and others.

[1] Address of the retiring vice-president and chairman of Section B—Physics, American Association for the Advancement of Science, Des Moines, Iowa, December, 1929.

These have come crowding on each other's heels with ever-increasing unmannerliness, until the average physicist, for whom I venture to speak, flounders in bewilderment.

Are there not some general principles disclosed in all this welter of mathematical reconstruction in which the average physicist may find a sense of comparative peace, of some security that his endeavors to reconstruct his conceptual attitude will not have to begin over again next week and of some assurance that his program of future activity lies along lines of real significance?

One very broad generalization from past experience is that whenever we extend the domain of experiment we must be prepared for unexpected new facts. If we whole-heartedly accept this generalization as of real significance, important conclusions follow which apply to both our experimental activity and our conceptual outlook. On the experimental side it follows that every real extension of our present experimental range is worth while and necessary. An increase of accuracy of measurement constitutes an increase of range, so that any one who can increase the precision of any sort of measurement makes an important contribution. Not every one may be interested to increase by a factor of ten the precision of weighing, for example, but there are persons to whom this sort of refinement is congenial, and who can now pursue the line in which they are most skilful, while the others of us can enthusiastically applaud their skill.

Further, in formulating to ourselves what the present situation actually is, we should cultivate a more deliberate self-consciousness of the accuracy of our present experimental knowledge. This should get into our courses of instruction, certainly into those for our graduate students if not into the elementary courses. We should all know the limits of accuracy obtainable, for example, in measuring a length or a weight or an interval of time or a temperature; we should know how accurately the inverse square law of gravitation has been established, or how accurately the gravitation constant or the velocity of light or the gas constant has been determined.

Our point of view gives added importance not only to every increase of precision but also to other sorts of increase of the experimental range, and those who take a constitutional satisfaction in pushing our experiments to higher temperatures or to higher electric or magnetic fields or to higher pressures or to higher light intensity or to higher gravitational or accelerational fields may feel a renewed sense of the importance of their contribution. But although experiments over greater ranges or with increased precision acquire an increased importance, it does not follow that all results here are equally interesting or significant, or that the prospective experimenter can expect to make his choice with his eyes shut and be awakened by the knocking of the postman bearing the Nobel prize. The postman is more likely to knock at the door of the man who in extending our range reaches qualitatively new effects, such as the wave-like structure of matter.

On the conceptual side, perhaps the most important principle disclosed by our continual discovery of strange new facts whenever we extend the domain of experiment is that the actual experimental world transcends all our efforts to get into perfect mental contact with it. We must by now feel that it is a little naïve continually to hope that our last theoretical formulation will prove to have arrived at the long-sought goal, when in the past our hopes have been continually shattered by each new discovery. Acceptance of this situation carries with it the conviction that in the last analysis any adequate scheme of getting into touch with experiment can be only descriptive, for no theoretical scheme of explaining nature can be regarded as secure until verified by every possible experiment, and when every such possible experimental check has been applied, the theory degenerates into a description. The fact that every acceptable description of nature is rational might at first be thought to have some deeper significance, but I believe that analysis will show that this means simply that we refuse to accept any description which is not framed in such terms as to be adapted to our mentality, a requirement so inevitable as to be almost without significance.

Another suggestion which may be of value in our search for elements of stability in our attitude is afforded by our ever-increasing appreciation of the importance of the unavoidable subjective element in any account which we can give of our experience. We used to demand that the ultimate goal of physical theories should be nothing less than the discovery of the underlying realities. To-day our demand for reality is much less insistent, in large part because we are much less confident that the ultimate reality, which we thought to be our goal, has any meaning. The meaning to be attached to reality is to a large extent a personal matter and changes with time, but I believe it is fair to say that the sense in which every one used reality a few years ago and the sense in which the majority use it to-day has "uniqueness" as a minimum connotation. It would not have been admitted that two entirely different explanations of the universe could each be equally real, but to-day we see that uniqueness in an explanation is an impossible ideal, and the quest for reality, in so far as reality connotes uniqueness, must be abandoned as a meaningless quest. A sufficient basis for this change of attitude could be found in the proof of Poincaré that any

aggregation of phenomena, no matter how complicated, is always susceptible of an infinite number of purely mechanical explanations. The reason that we are not interested in giving an explanation of quantum phenomena on a purely mechanical basis is that any such explanation would involve the assumption of a prohibitively complicated amount of detail concealed beyond the reach of any contact with experiment. It is natural, therefore, to find that the demand that our theories reproduce reality is becoming replaced by the demand of convenience and simplicity. Another requirement in a satisfactory theory has recently been much emphasized by Heisenberg and his school, namely, that our theories should contain only observable quantities. This involves so modifying the concept of reality as to make it closely associated with the possibility of direct observation. But although this alternative formulation of the reality concept has at first a most satisfying aspect, I believe that in the actual working out it is less satisfactory than in anticipation. In fact, I am inclined to think that Heisenberg's demand that only observable quantities enter the theory played only a suggestive rôle in leading to one of the many possible solutions and was as sterile in actually compelling the adoption of his form of theory as was the corresponding demand of Einstein that the law of gravitation be written in an invariant form. For if one examines how the principle works in practice, it will be seen that all that is demanded is that the raw material which is fed into the calculating machine and the final results which are taken out shall connect with direct observations. All the intermediate processes and operations, the internal pistons and gears of the theory, have as much the character of pure inventions as anything which Poincaré might have proposed. However much one might have been inclined fifty years ago to see some warrant for ascribing physical reality to the internal processes of a theory because of its success in meeting the observed situation, certainly no one of the present generation will be capable of so naïve an attitude after our illuminating experience of the physical equivalence of the matrix calculus and the wave mechanics.

As a consequence of all this, the attitude of the physicist to-day is changing toward mathematical theory. As a whole, he takes it far less seriously, recognizes that it contains less of reality and more of a purely suggestive character than he had realized, and lays more emphasis on the demands of simplicity and convenience. There are puzzling questions to be answered and instinctive reactions to be overcome in adopting this point of view. It is hard to resist the conviction that there is some deep underlying significance in the fact that the mathematical operations of the matrix calculus of Heisenberg, for example, make such wonderful contact with experiment. But what the precise significance of this may be eludes formulation, and in the meantime the physicist must fortify himself with somewhat skeptical considerations like the following:

> On the one hand, mathematics is a study of certain aspects of the human thinking process; on the other hand, when we make ourselves master of a physical situation, we so arrange the data as to conform to the demands of our thinking process. It would seem probable, therefore, that merely in arranging the subject in a form suitable for discussion we have already introduced the mathematics—the mathematics is unavoidably introduced by our treatment, and it is inevitable that mathematical principles appear to rule nature.

What do these general considerations have to do with our program of action. In the first place, we are going to be exceedingly cautious in ascribing any finality to the details of the present mathematical theories. There is among the younger and more enthusiastic members of the physical community a tendency to regard the present theories as final which the more sedate members must combat, even in the face of all the successes of the present theories. One of the most certain lessons of the past is that no amount of success in the youth of a theory is any guarantee of a hale and hearty old age; this is to be expected and is a consequence of the transcendence of nature. Against this view, an enthusiastic protagonist of the new theories might with considerable justification, I believe, urge that there are certain elements of genuine novelty in our present outlook which offer a basis for the belief that we may be on the point of breaking away from our often-repeated cycles of revision and reformulation. But although there may be distinctly encouraging signs of a brighter future, I believe that nevertheless there are specific elements of weakness in the present situation which justify the suspicion that our present theories still need thoroughgoing modification. Perhaps one of the most serious weaknesses of the present theory is the way in which it deals with static effects. One important consequence of the Heisenberg principle is that an electron can not stand still, yet a potential energy is substituted into the fundamental equations of the theory which retains the old fiction of an inverse square force emanating from a stationary center, and which palpably has no meaning in terms of direct experiment. Such a thing is entirely opposed to the spirit of our new outlook. We are to expect that presently the apparently static inverse square law of force will be described in statistical terms. Another suggestion that the present theory marks only a half-way stage of progress is to be found in its treatment of the universal constants, such as the gravitation constant,

the velocity of light, the charge and the mass of the electron and the quantum h. No one, I suppose, is yet so pessimistic as to give up the hope that eventually we will be able to give an account of these constants, instead of always having to carry them in our equations as elements imposed from without. One may also hope to have sometime a theory of the equality of the charge on proton and electron.

Perhaps these misgivings about the permanence of our present theories seem of too vague and general a character to be of much significance, but I believe that in the past we have been many times too willing to forget any very broad general objections to which our theories may have been subject, in our satisfaction with their success in dealing with fairly wide classes of special phenomena. For example, the fact that the classical principle of equipartition of energy demanded that the atoms be mathematically rigid should in itself have been sufficient to show that any attempt to reduce all action to pure mechanics was certain to fail.

All this must not be allowed in any way to minimize our conviction of the very great importance of carrying through the analysis of all possible consequences of our many new mathematical points of view, but it does suggest that many physicists who are not professionally interested in making new contributions to new mathematical developments, but rather want to understand what there is of permanent significance in present developments, may with a clear conscience omit to work through the details of much of recent mathematics, in the conviction that it is of more or less transient character. But apart from the mathematical details, and perhaps sometimes not intimately connected with them, there are certain broad qualitative points of view characteristic of the new theories which every physicist should grasp and incorporate into his thinking. Perhaps the two most important of these points of view are (1) that the measurable properties of electrons embrace some phenomena which we find convenient to describe in terms of the wave phenomena of ordinary experience, in addition to the older and more familiar phenomena which we have satisfactorily dealt with in terms of a particle picture; and (2) that there is some essential limitation to the sorts of measurement that can be made simultaneously on elementary things, which is formulated in Heisenberg's principle of uncertainty. In speaking of these points of view as two I do not wish to imply that they are not logically connected, for they are very intimately related.

It is possible to direct just criticism at the mathematical deduction of these two principles from the logical premises. The wave mechanics is open to various objections, one of the chief of which to my mind is that it provides no way of dealing with transient phenomena; this fact constitutes to a certain extent a failure of the fundamental principle, for it is only transient phenomena which are directly observed. Further, the deduction of the Heisenberg principle as a necessary consequence of the fundamental assumptions has failed to satisfy many and does indeed seem to contain a certain feature inserted arbitrarily into the theory. But I believe that in spite of these criticisms these two points of view transcend the mathematics by which they were derived, and that, inspired and guided by the mathematics, we have come upon a point of view which is of more permanent value than the mathematics itself.

These two points of view, if I understand correctly the claims made for them, should be sufficient, in conjunction with other physical knowledge which we already have, to determine the nature of the elementary processes and entities which analysis of our physical experience discloses to us. Here we reach the actual frontiers of physical exploration, and doubtless the most fundamental problem confronting us is to acquire understanding of these things. The more complicated things, such as the chemical properties of molecules, involve processes of mathematical synthesis which we need not expect to grasp intuitively and which will not be completely worked out for some time in the future, but of the qualitative nature of the underlying elemental processes and entities all physicists should now attempt to acquire some intuitive command. Since the new theories are formulated so as to be consistent with the cardinal principle that the properties of a thing have no meaning which is not contained in some describable experience, our intuitions should be able to tell us what to expect in various experimental situations involving elementary things. This does not mean that the experience in terms of which our intuition thinks is necessarily an experience so closely connected with actuality that we could go into the laboratory and make the experiments, but the experiments must be such as are allowed in principle by the new theories. For instance, we can conceive ourselves in principle determining the frequency of a single photon by finding the location on a photographic plate of a single developed grain exposed in a spectroscope of infinite resolving power, although we may perfectly well recognize that to make such a measurement is beyond our present experimental skill. Our intuitive grasp of an elementary situation may, then, be tested by our ability to describe what to expect in terms of conceptual experiments. I believe that in the devising and discussing of such conceptual experiments there is an important field which may be cultivated, particularly at the present time, with much profit. It should be possible to build up a formal structure in which the properties of photons and electrons and

other elemental things, such as quantum interactions, are described in terms of conceptual experiments, and from simple properties deduce more complicated properties in much the fashion of Euclid. In fact, the resemblance between this ideal and Euclid is a rather close one, for that part of the analysis of Euclid which consists in moving figures about and comparing them by superposition amounts to nothing more than conceptual experiments with geometrical figures. A systematic development of the conceptual experiment would be found by many, I believe, to give a more illuminating insight than a painful acquisition of the details of the present mathematical picture.

As suggestive of what may be done here, I append a list of questions which are to be answered in terms of conceptual experiments allowed by the new point of view.

(1) Are experiments on single "naked" electrons possible? How may one be sure that he has a single electron in his apparatus? Are there any methods of detecting the presence of an electron that do not demand that the electron be traveling with fairly high velocity? Can a stationary electron be detected? Millikan.

(2) How may the charge of a single electron be measured? Is there any theoretical limit to the accuracy with which a single measurement of charge may be made? Or is an accurate value of e obtainable only from statistical measurement?

(3) What is the evidence that an electron has independent existence in empty space? May one electron stream receive a deflection on impinging on another?

(4) Is the equivalence of the charge on electrons and protons a statistical or an individual effect? How accurately may the charge of an individual electron and proton be proved equal?

(5) How may the magnetic moment of a single electron be found?

(6) What properties may an electron have simultaneously? We know that it can not simultaneously have position and velocity. May the charge, the mass, the momentum and the energy be simultaneously determined?

(7) Is a single electron subject to a gravitational field?

(8) To what extent does an electron have identity? May it be observed continuously, or is there a minimum time between successive observations?

(9) How do the measurable properties of an electron in those places where, according to the wave mechanics, the kinetic energy is negative differ from those of a classical electron?

(10) How may the frequency of a single photon be measured?

(11) May the frequency of a single photon be measured without at the same time compelling it to have some direction, that is, are frequency and direction independent properties?

(12) May the energy of a single photon be measured independently of its frequency?

(13) Does a single photon have a plane of polarization, that is, may the plane of polarization of a single photon be measured? (I have been able to discover no method of doing this.)

(14) Can the velocity of a single photon be measured? All experimental determinations of the velocity of light have been essentially measurements on a steady state.

(15) What experimental method is there of detecting the motion of a single photon?

(16) How many properties does an individual photon have simultaneously? For example, may the velocity, the frequency, the direction, the momentum and the energy be measured simultaneously?

(17) Does a photon have independent existence in empty space? Can two crossed streams of photons be made to disturb each other?

(18) To what extent does a photon have identity?

(19) Is there any method by which the emission of a photon from an atom may be detected which does not involve receiving the emitted photon?

(20) What sort of a constant is h? May it be determined from a single quantum process, or is it essentially statistical? The six methods for determining h listed by Birge are all essentially statistical.

(21) Is there any evidence that two quantum processes ever interfere with each other, or that one begins before the other has ended?

It will very probably be found that the answers to some of these questions can not at present be given without a rather intimate acquaintance with mathematical theory, but I believe that this is merely a temporary phase and that ultimately we shall be able to demand that our theories be so formulated that we can answer these and other questions intuitively without recourse to formal mathematics. In the meantime, I believe that any one who attempts to devise the conceptual experiments by which these questions may be answered is not only increasing his own understanding of fundamentals but is also making an important contribution to physical progress.

THE MUTUAL INFLUENCE OF ORGANIC COMPOUNDS IN THE ANIMAL BODY

By Professor F. KNOOP
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SEVERAL papers read at the Thirteenth International Physiological Congress in Boston impressed the writer as showing that the ideas of the reciprocal influences between organic substances in the sense of

an exchange of energy between each other are not sufficiently known. It seems to me to be important that the simple basis of this kind of consideration should become better appreciated by biologists, and I should like to try to collect into a generally comprehensible form what it was impossible to say in the short time allowed for discussion during the meetings.

Many processes of reduction occur in the animal body for which energy requirements can not, as in plants, be covered by the direct transformation of the rays of the sun. This energy may be supplied by the heat liberated by any process of combustion, as exemplified by fat-production following feeding carbohydrates exclusively. If the organism reduces a ketone to an alcohol, it needs H_2, which is not directly at its disposal. Therefore, another organic compound, undergoing oxidation, gives H_2 not to oxygen, but to the reducible substance and transfers with it part of its heat of combustion. The simplest example of this kind is the disproportion of 2 mol. of aldehyde into 1 mol. of alcohol and 1 mol. of acid (Canizzaro). The heats of combustion in this case are for acetaldehyde:

$$\underset{\text{2 mol. Aldehyde}}{(2 \times 282 =)564 \text{ Cal.}} = \underset{\text{Alcohol}}{340 \text{ Cal.}} + \underset{\text{Acid}}{209 \text{ Cal.}} + 15 \text{ free Cal.}$$

One molecule of alcohol gains 58 calories from the acid, which contains 75 calories less than the aldehyde. The 15 remaining calories are liberated as heat, thus indicating why the process may take place spontaneously according to general physical laws and can well be accelerated by catalysts. Parnas in Hofmeister's Institute has shown that there are ferments in the animal body which catalyze this reaction with numerous aldehydes.

Wieland has shown in his theory of dehydrogenation that for many oxidative processes the primary and essential reaction is not the entrance of oxygen, but the removal of hydrogen. This H_2 is mostly bound by O_2 when the oxidation takes place in a high O_2 concentration. And since Wieland could demonstrate that $H_2 + O_2$ form H_2O_2, so he proved definitely the value of his idea and, incidentally, the purpose of katalase. Wieland has shown that other substances than oxygen can bind this hydrogen in experiments which agree with those of Thunberg, Schardinger and others. Methylene-blue quinones and analogous substances act here principally as in Canizzaro's reaction—the aldehyde which takes up hydrogen is converted into alcohol. One substance is hydrogenated by another one: the first takes up energy which is provided by the second. The mutual relations of these two substances are believed to be analogous to those existing between many compounds *in vivo* and to typify the energy exchanges of many biological reactions.

The living organism is able to facilitate such oxido-reductions between substances of widely differing kind. It has been known for a long time that aldehydes can be converted into alcohols. The H_2 can only be supplied by other organic bodies. For the hydrogenation of unsaturated linkages and for the transformation even of carbonyl to methylene groups exact chemical proofs can be found in our first paper on β-oxidation. Benzallaevulinic acid is transformed by the dog into phenyl-α-oxybutyric acid, a change requiring both oxidation and hydrogenation of double bonds and carbonyl groups:

$$C_6H_5 \cdot CH = CH \cdot CO \cdot CH_2 \cdot CH_2 \cdot COOH \rightleftarrows$$
$$C_6H_5 \cdot CH_2 \cdot CH_2 \cdot CHOH \cdot COOH[+ C_2 \cdot {}^2]$$ [1]

Such a transference of hydrogen from one compound to another can also be effected by a third substance. It will always remain as a memorable contribution by Hopkins that he found in glutathione a substance illustrating this function. The SH-group of glutathione is able to furnish this hydrogen or to accept it, and so can reduce or oxidize other substances. Glutathione now is made responsible for all kinds of reductions, even where evidence is lacking. But there is no reason to believe that glutathione is a unique substance of this kind: certainly such processes are facilitated in the body by many compounds of different composition. We have long known that iron in its two stages of oxidation can perform the same function. Organic substances can doubtless act similarly.

Most oxy- and ketonic acids are mutually interconvertible; acetoacetic acid, for example, oxidizes another substance if it robs it of hydrogen and is converted into oxybutyric acid; on the other hand, acetoacetic acid reduces it, if the added hydrogen is transferred to other bodies. The foregoing example can not perhaps be regarded as proved in all details, but other analogous reactions are known. An interesting example is offered by the amino acids, which have thus a new and important function.

Neubauer has shown that the first phase of the decomposition of amino acids in the animal body is an oxidative transformation to a ketonic acid and ammonia. And we have been able to analyze the reaction further after we found the reaction to be reversible.[2] It can for the present be ignored whether an amino-, oxyamino- or an α-β unsaturated acid is first formed—the important fact is that the process of oxidation involving loss of hydrogen can be made retrogressive and the amino acids can be reformed.

[1] *Hoppe Seyler's Ztschr. für physiolog. Chemie*, 89: 141, 1914.

[2] *Idem*, 67: 489, 1914; 71: 252, 1911.

When this was demonstrated, an old principle was abandoned according to which the chemistry of plants and animals could be fundamentally differentiated by the assumption that the plants can assimilate inorganic nitrogen, but that animals can not do so. This new discovery produced at first only a great deal of discussion concerning its quantitative value, which may be small. But the principle involved in this reaction was ignored. Therefore we have examined more closely the ease with which reduction occurs in this reaction. We have found the surprising result that the reaction takes place very easily. It is well known that a ketonic acid is easily reduced to an oxy acid and that the body can do the same thing. But in the presence of only a small amount of ammonia, and the theoretical quantity of hydrogen at room temperature, an α-ketonic acid yields α-amino acids, and little or no oxy acid—which was quite a new result. This synthesis of an amino acid needs less energy than the synthesis of the oxy acid, and this is probably the reason why the latter are usually formed in small amounts. Extended experiments with other ketonic acids and with secondary and tertiary amine bases clearly indicate the general form of the synthesis of the amino acids in nature, which certainly does not follow the same lines that chemists have found in their laboratories. The reaction can be accelerated by catalysts so that 75 per cent. yields of pure amino acids can be obtained at room temperature in a few minutes.[3]

Even if the reaction proceeds only to a limited extent, the system ketonic acid plus ammonia must be able to take up hydrogen from other substances and to oxidize them with regeneration of the original amino acid. The constant combustion of protein in the body provides continually for the new formation of this system, and we have concluded that this reversibility[4] of the first oxidative phase of the degradation of amino acids entitles them to the rôle first of hydrogen-donators and secondly of hydrogen-acceptors. They act after all in principle like Hopkins glutathione.[5] We have further found that this same system can oxidize cystein to cystin with simultaneous formation of the original amino acid.[6] And Bergmann and Enslin[7] have now confirmed the truth of our conclusions. They have demonstrated that the small differences in the redoxpotentials (Michaelis) of individual amino acids suffice to enable one amino acid undergoing the first phase of decomposition to oxidize a second one, with re-formation of the original amino acid. This relationship has been demonstrated between phenylalanin (as phenylpyruvic acid plus ammonia) and aspartic acid. . .[7]

We must therefore attribute to a great number of organic compounds the capacity to transfer energy from other substances or to furnish it—to oxidize or reduce them. One may be allowed to generalize from these ideas and say that probably every organic substance in the body has in relation to every other one a certain redoxpotential, which will vary according to the conditions, for example, of the pH, of temperature, of O_2 concentration, of the catalysts present, etc. And since the animal can vary these external conditions to a considerable extent, we believe that in the living organism the processes of synthesis and decomposition are regulated by a continuous conflict of all kinds of substances with each other. The extraordinary fineness of the chemical adjustments of the animal body must be referred to this multiplicity of reciprocal actions between all the products of intermediary metabolism. It may be noted that according to this view chemical regulation of the organism's activity may be regarded for the moment to some extent independent of the essential supply of oxygen. We have for long urged that more attention be given to the reciprocal influences of organic substances on one another. In this way we shall acquire a greater knowledge of the chemical abilities of the living cell. I hope that the physical-chemical kind of treatment which Michaelis gives in his new book on redoxpotentials will give us great help. Certainly it emphasizes from a new side the importance of these questions to chemists and physiologists. Reactions such as those referred to are of great general biological interest; for example, the capacity to transform the groups of the main foodstuffs into one another.[8] Thus, the synthesis of fat from protein or sugar, the amino acids of proteins from carbohydrates, etc., gives to animals a wide independence as to the kind of food which is essential.[9] Without these chemical capabilities a much greater amount of animal life would die continually, and especially for the chemist of the anoxybiotics these abilities are of the greatest importance. The animal body can perform all these transformations only because it is enabled to bring in reaction all types of organic substances in a manner which can be here discussed only in general outlines.

[3] *Idem*, 148: 294, 1925.

[4] The word reversible is used only in the sense of "biologically reversible" and not in the strict physicochemical sense.

[5] *Hoppe Seyler's Ztschr. für physiolog. Chemie*, 148: 302, 1925.

[6] *Idem*, 170: 186, 1927.

[7] *Idem*, 174: 76, 1928.

[8] *Klin. Wochenschrift*, 2: 60, 1923.

[9] *Münch. med. Wochenschrift*, 73: 2151, 1926.

SCIENTIFIC NAMES

By Professor JAMES G. NEEDHAM
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A SMOULDERING impulse to say something more about the wearisome subject of nomenclature suddenly burst into flame the other day when a friend of mine brought to my attention the two following names: *Brachyuropushkydermatogammarus grewinglii mnemonotus* Dybowski and *Microstomaticoichthyoborus bashford-deani* Nicholls and Griscom. The first is a little Amphipod crustacean; the second is a very small fish. If I work with the groups to which they belong, I shall have to handle these names, and I object!

Almost two decades have passed since in these columns[1] I raised the question: Whether there is not a better way of disposing of our nomenclatural trouble than by making it as burdensome as possible, and then making it permanent? Several stalwart defenders of the established order arose to condemn[2] in these columns a simplification that I then proposed, but not one of them ventured a word on the main question. There was no response at all in print, though I did receive a few commendatory letters. I suppose it was because my proposal seemed to threaten the sacred Law of Priority. Hands off it! Give it time. Stability and agreement were just around the corner.

Now there has been time to show what our efforts toward stability alone will do for us. The results are apparent to all. The load of scientific names becomes ever more burdensome and the nomenclatural experts seem to be wholly bent on making that load permanent. And in the name of all who need to use scientific names, and who are not systematic specialists, I object. I ask again whether simplification is not possible.

Biological nomenclature has grown burdensome in part because of increase of knowledge, but in a far larger part because of the artificial premium put upon the giving of names—a premium the like of which is not awarded in any other line of scientific work. Darwin put his finger on one source of our trouble when he wrote:[3]

> . . . I have been led of late to reflect much on the subject of naming, and I have come to a fixed opinion that the plan of the first describer's name being appended for perpetuity to a species has been the greatest curse to Natural History. I feel sure that as long as species-mongers have their vanity tickled by seeing their own names appended to a species, because they miserably described it in two or three lines, we shall have the same *vast* amount of bad work as at present, and which is enough to dishearten any man who is willing to work out any branch with care and time.

But that was only a beginning. The name of the genus also offered a point of attachment for memorials to describers. And it has been availed of fully. Existing rules and practices hold out such promises as this: Coin for a new genus any name that has not been published; couple it with the name of any valid species; give any kind of a description or no description at all; publish it—and you are entitled to write your own name after it, and all who come after you are expected to use it so forever. Was ever a promise of eternal remembrance so cheap!

Little wonder that generic names have multiplied beyond reason! Such devices as prefixes and suffixes enabled the genus-grinders to work faster, and when these began to fail, compounding them followed. Felt and Bishop pointed out that some "have been driven to the making of extremely long names in order to lessen the probability of creating synonyms!"

Nowadays the coining of new generic names has become so common that it has lost some of its charm, and the true experts in taxonomy are devoting themselves assiduously to the coining of new family and subfamily names, for, under the newer practices these, also, may carry forever the names of their inventors. The experts now go gunning for bigger game. And, at the present rate, we shall soon have in a large part of the system a subfamily for every genus. When these names represent new taxonomic discoveries they are perhaps excusable, but often they are merely *new names* applied to groups already set apart by previous workers.[4]

Subfamily names need not detain us here. They are the concern of the systematic specialist. Let those who want them have them. It is the scientific name of the species that we all use, the name by which it is known everywhere and in all languages alike. This name is in two parts, genus and species—like surname and given name of a person; this since the day of Linnaeus. Strange things have

[1] SCIENCE, 32: 296, 1910.

[2] T. D. A. Cockerell, SCIENCE, 32: 795, 1910; D. S. Jordan, SCIENCE, 33: 370, 1911; *et al.*

[3] Quoted in "Life and Letters of Charles Darwin," by F. Darwin, p. 334.

[4] If I be taken to task again, as I was recently by my friend Dr. Jordan (SCIENCE, 66: 14, 1927) for disrespect to taxonomists, "a signally unselfish and conscientious group," my complete answer is: Am not I myself, in a small way, a describer of new genera and species?

been happening to his simple helpful system of binomials. His names were pronounceable and fit. Forty years ago when I argued with some of my friends in favor of *appropriate* names, I was silenced with the dictum: "A name is a name and not a definition." His names were brief; but now in all the better-known groups, trinomials are the rule and quadrinominals are becoming common. I myself trudge along lonesomely at the tail of the procession. For me, if a form is distinct enough to bear a name at all, that name shall be a binomial. And I meet the queries of my taxonomic friends with another dictum: "A name is a name and not a treatise on relationships."[5]

Some editors are now so well disciplined in correct usage that they require the addition of the name of first describer throughout, in all the manuscripts that they accept for publication. When there are joint authors and tri- and quadrinomials this makes a fine string of names.[6] Some add to original describer's the name of the man who puts the thing in another genus than that in which it first reposed. I do not do this, and I justify my slowness with another dictum of my own: *A name is a name and not a memorial inscription.*

So, I agree that a name is a name. But, though a name is only a name it may yet be a helpful thing or a very positive hindrance. The two names quoted in my first paragraph are examples of the latter. They are far worse than anything pre-Linnaean.[7]

My point is not that names are so numerous—they must be as numerous as the species—but that they are so inanely foolish as to hinder the progress of biological science. How many species names there may be does not concern the general worker. He needs as many of them as there are kinds of plants or animals before him, and the others do not bother him. But the number of generic names does concern him; for if he has to learn a new genus name for almost every species, that name is no aid at all, but only an added burden on his memory. Furthermore, it is obscuring, not clarifying. The original purpose of the generic name was to tie the species up in convenient bundles so that the mind might the more easily deal with them. The general worker needs only enough generic names to cover readily distinguishable groups. Modern infinitesimally split genera, based on differences that only a specialist can see, tend to obscure affinities. The taxonomic specialist is apt to think of large genera as "unwieldy"; but the real unwieldiness lies in the excessive array of handles with which he supplies his little bits of intellectual luggage.[8]

In 1910 I tried to start a discussion of the problem of simplifying our nomenclature. I suggested numbering species; but numbering does not accord with our traditional habit of naming things. That was not a good suggestion. Let it pass. I now try again.

I suggest a return to simple binomials with fewer genera and a standard name list for the use of all who deal with plants and animals otherwise than as taxonomic specialists. Such a list would present the little crustacean mentioned in my first paragraph not as *Brachyuropushkydermatogammarus grewinglii mnemonotus* Dybowski, but as *Gammarus mnemonotus* and nothing more.

Let the existing system stand for the systematists. Let it grow and flourish. Let the splitters have their revel. The *mihi* itch is such a delightful disease, I would by no means deprive my worthy systematic colleagues of the pleasure they find in scratching. But let us have simpler names for common use.

In these pages I am not proposing a plan, but an inquiry to develop a plan. It is only by agreement that the present system came into existence. To say that biologists might not yet be able to agree upon something a little less distressing and less a hindrance to biological science would seem to me very like a confession of professional idiocy.

There are, however, four provisions I should hope for in the new undertaking:

1. A preliminary report to competent psychologists as to desirable and necessary name-limitation.
2. Cooperation between botanists and zoologists.[9]
3. Members-at-large in the name-choosing body, to secure a measure of uniformity.
4. Members serving only for the groups in which they are taxonomic specialists, to bring to bear a working knowledge of the group and of its literature and tradition.
5. Some better method of obtaining the opinion

[5] Whether Marcus Smith is first or second cousin to John Smith his name need not explain.

[6] Here are the up-to-date names of five species of bumblebees: *Cullumanobombus silantjeivi semenoir-tianshanskyi* Shorikov; *Pratobombus lapponicus kamtshaticus occultodistinctus* Shorikov; *Agrobombus solstitialis subbaicalensis insipidioides* Vogt; *Agrobombus agrorum parcuorum subdrenowskianus* Vogt; *Pratobombombus jonellus atrocorbiculosus sparre-schneideri* Vogt.

[7] If any one thinks that such monstrosities in names are isolated cases let him read the four pages of generic names derived from personal names in Palmer's "Index Generum Mammalium" (*N. Amer. Fauna*, no. 23, pp. 48-51, 1904): Asmithwoodwardia, Ernestohaeckelia, Ricardolydekkeria, etc.

[8] The true aim of taxonomy was well stated by J. Chester Bradley in SCIENCE, 66: 103, 1927.

[9] Without this, we who are ecologists may have to go on forever writing the names of plants one way and the names of animals another. Must we capitalize and parenthesize and eulogize first describers and last shifters in two ways, merely because the plant and animal taxonomists have neglected to get together? We do not like it. It is not rational.

of zoologists than the *viva-voce* vote of the crowded sessions of an international congress.

I speak for the conscientious teachers who seek to give their pupils some contact with biological literature. Confusion of names, absurdity of names, apalling length of names waste their time and dull the interest of their pupils. In some quarters it seems to be expected of the teachers that they shall meekly and apologetically explain to their pupils that all this foolishness is due to the vastness of plant and animal creation. But it is not so. The fact that there are more than a million species of animals in the world does not of itself necessitate that the bird in my hand shall bear a harder name.

I am well aware that it will be no easy task to find a way out of the existing confusion. Good judgment, expert knowledge, human sympathy, hard labor and long patience will all be required. When these have been applied we may hope for something better. It surely is not well to have scientific effort so organized as to reward mistakes and to preclude the elimination of errors.

SCIENTIFIC EVENTS

ENGINEERS IN AMERICAN LIFE

Men of science are assuming a dominant position in American life, Lawrence W. Wallace, of Washington, executive secretary of the American Engineering Council, said in an address at the recent annual meeting of the American Society of Mechanical Engineers in New York. By supplementing with broad humanistic and scholarly interests the technical genius responsible for the "Machine Age," they are becoming a controlling force in culture and in politics no less than in commerce and industry, in finance, in education, and in national defense, Mr. Wallace asserted, making public the results of a survey of "Engineers in American Life" which he conducted in association with Joshua Eyre Hannum, research engineer of New York.

Of the 28,805 "notable living men and women of the United States" named in "Who's Who in America" (1928–1929), 2,858 were engineers and architects. They received 1,417 academic degrees in branches of learning other than science, as well as 2,497 scientific degrees. They are members of 1,138 associations, conferences, boards and commissions, half of which are non-technical.

They hold 4,785 official positions in 3,928 organizations, of which number 2,993 are industrial and commercial companies. They occupy the position of president in 1,128 industrial and commercial organizations, 72 engineering firms, 68 banks and trust companies and 23 colleges and universities.

Among these 2,858 engineers and architects there are, or have been, 10 governors, 13 members of Congress, 2 members of the Cabinet, and the president of the United States. Five hundred and eighty-eight of these men hold 905 memberships in Phi Beta Kappa, Phi Kappa Phi, Sigma Xi and Tau Beta Pi, the four honorary fraternities in which membership connotes outstanding achievement in various fields.

The men studied hold membership in 1,138 associations, commissions, conferences, and the like, half of which are non-technical. The activities of these organizations touch practically every interest of mankind, and they are not restricted geographically, but are located in many parts of the world.

Nearly 40 per cent. of the group are officers of industrial and commercial companies. Of the entire group, 37.1 per cent. are available for consultation, 34.8 per cent. have made contributions to scientific and technical literature and 6.9 per cent. have been editors of technical papers.

In the fields of public service scientific men have made important inroads. Over one half of the men under consideration have served or are serving municipal, state or federal governments. City governments have been served by 208 of these men. Among them have been 28 city engineers, 18 mayors, 6 city managers, 3 chiefs of police and 2 superintendents of city schools.

State governments have engaged the efforts of 269 of the group, 59 of whom are now in state work. There have been 10 governors, 5 legislators and a state district attorney among them. The present governors of Delaware, Utah, Wyoming, New Jersey, Alaska and Alabama are engineers. The governors of North Carolina and Indiana hold engineering degrees.

In our federal government, the president of the United States, the secretary of the interior, the secretary of commerce and the director of the budget are among the men of science holding important positions. To name the various branches of the federal government in which engineers hold responsible offices would be to catalogue the activities of the government. Suffice it to say that no other group is more influential in shaping the destiny of the nation.

DENTAL COUNCIL ON MATERIA MEDICA AND THERAPEUTICS

The American Dental Association announces the formation of a proposed council to deal with dental materia medica and therapeutics.

The functions of the council will be to aid the dental profession to further rationalize its materia medica and therapeutics by fostering a spirit of chemical mindedness on the part of the dental profession, and to protect the dental profession and public against fraud, undesirable secrecy and objectionable advertising in connection with proprietary dental remedies. For the immediate future, the council will confine its attention to the examination of drugs and dental proprietaries for inclusion in an "accepted list." It is planned, however, that the scope of the council will eventually be enlarged to consider ceramic and metallurgical products and physical therapeutic apparatus in the dental field. The endeavors of the council will also be directed toward an improvement in pharmacological and biochemical instruction in dental colleges.

Invitations to serve on the council have been accepted by the following:

Paul J. Hanzlik, professor and head of the department of pharmacology, School of Medicine, Stanford University.

Percy R. Howe, Thomas Alexander Forsyth professor of dental science, Harvard University, and director, Forsyth Dental Infirmary for Children, Boston, Massachusetts.

Milan A. Logan, instructor, department of biological chemistry, Harvard Medical School.

Arno B. Luckhardt, professor of physiology, University of Chicago.

John A. Marshall, associate professor of dental pathology and biochemistry, College of Dentistry, University of California.

Victor C. Myers, professor of biochemistry, Western Reserve University.

John F. Norton, chief of laboratories, department of health, Detroit, Michigan, formerly professor of bacteriology and hygiene, University of Chicago.

U. G. Rickert, professor of physiological chemistry, hygiene and therapeutics, School of Dentistry, University of Michigan.

Harold S. Smith, practicing dentist, Chicago, Illinois.

As *Ex-officio* members:

C. N. Johnson, editor of the *Journal* of the American Dental Association.

Harry B. Pinney, secretary of the American Dental Association.

Samuel M. Gordon, American Dental Association chemist and former National Research Council Fellow in the Biological Sciences, has been appointed secretary of the council.

The first meeting of the council was held in Chicago on January 3 and 4, 1930. Examinations of purely dental nostrums will be dealt with by the Bureau of Chemistry of the American Dental Association as heretofore.

GOLD MEDALS OF THE MASSACHUSETTS HORTICULTURAL SOCIETY

TRUSTEES of the Massachusetts Horticultural Society have made their final survey for the centennial year and awarded twenty centennial gold medals, eighteen centennial silver medals, six large gold medals and made two special awards. The gold medals, as announced in the *Boston Transcript*, are as follows:

Albert C. Burrage, president of the Massachusetts Horticultural Society, for eminent service in horticulture.

T. A. Havemeyer, president of the Horticultural Society of New York, for his many years of distinguished service to horticulture.

F. R. Newbold, secretary of the Horticultural Society of New York, for his many years of distinguished service to horticulture.

John C. Wister, secretary of the Pennsylvania Horticultural Society, for his many years of distinguished service to horticulture, especially through his writings and lectures.

Worcester County Horticultural Society, organized in 1840 and ever since that date a potent factor in the advancement of horticulture in Massachusetts.

Dr. Henry P. Walcott, as a fitting tribute to a former president of the Massachusetts Horticultural Society.

Nathaniel T. Kidder, as a fitting tribute to a former president of the Massachusetts Horticultural Society.

General Francis H. Appleton, as a fitting tribute to a former president of the Massachusetts Horticultural Society.

William C. Endicott, as a fitting tribute to a former president of the Massachusetts Horticultural Society.

Professor Oakes Ames, for his service to orchidology. The botany of the orchid has been Professor Ames's life study, and no man stands higher in this field.

Professor Ernest H. Wilson, for his inspirational books. Besides being the greatest of living plant hunters, Mr. Wilson has a rare capacity for transferring his knowledge to paper.

Olmsted Brothers, Brookline, for their work in raising the standard of landscape architecture and especially in the development of public parks. There is hardly a corner of the country where their influence has not been felt.

Harlan P. Kelsey, East Boxford, for his work in behalf of better horticultural nomenclature. He was New England's representative on the committee responsible for standardized plant names.

Alexander Montgomery, Hadley, for his work in originating new roses. The fame which Hadley, Crusader, Templar and Talisman have brought to the man who originated them is shared with the state in which he lives.

Mrs. Susan D. McKelvey, for her notable monograph on the lilac, the most important work of the kind ever undertaken in this country.

Mrs. S. V. R. Crosby, for her work in the conservation of wild flowers. Her high purpose and practical methods

as president of the New England Wild Flower Preservation Society have won the deepest respect and appreciation.

Miss Marian Roby Case, for her work with boys at Hillcrest Gardens. For twenty years Miss Case has devoted her time and fortune to an experiment in education and horticulture which has proved unique and has attracted wide attention.

Kidder, Peabody & Company, Boston, for demonstrating the fact that a beauty spot can be made in the heart of a great city.

William Filene's Sons Company, Boston, for the example set by them in their consistent use of window boxes on business houses.

Professor B. L. Robinson, for his eminent service to botany. As head of the Gray Herbarium, Professor Robinson has carried on much important work with distinguished success.

PRESENTATION TO PROFESSOR NOVY

FRIENDS, colleagues and students of Dr. Frederick G. Novy, since 1902 professor of bacteriology at the University of Michigan, united on the occasion of his sixty-fifth birthday in the presentation of a full-sized oil portrait, painted by J. E. Weiss, and a volume containing two hundred letters of greeting from scientific men throughout the world.

Dr. Aldred Scott Warthin, head of the department of pathology, in presenting the portrait, said:

> We are assembled here to render richly deserved honor to a distinguished colleague, who for 43 years has been a worthy and faithful servant of the university. Not only as an original and forceful teacher, but much more as a scientific investigator, he has added luster to the fame of the university and medical school. More than any other single member of its faculties he has contributed to their international reputation. His name is known in all lands where modern medical science has a foothold, and it will be perpetuated to coming generations through the many imprints it has made upon the terminology of bacteriological science. The fame of the university can rise no higher than that of the individual members of its faculties. It is a collective fame, increasing from generation to generation as successive men of note follow one another in its professorships. It would seem fitting that an effort be made to perpetuate the memory of those who, in their day, gave of their best, in contributing to the university's intellectual development. It is with this object in view, that the colleagues and old students present this portrait to the university, of the man they delight to honor, Professor Frederick G. Novy.

In reply, as he accepted the painting, Dr. Ruthven said:

> We are met to-day to honor a man famed as a teacher, who has to an enviable extent contributed to the progress of science and to the advancement of scholarship at the University of Michigan, a man, moreover, who as our colleague has by his example and personality endeared himself to us, inspired us in our labors, and made us proud to have known him in his work. In honoring him we are not only giving him his due but we are also indicating our belief that the university stands for creative scholarship as well as for sound teaching. We will place this emblem with the prayer that it may inspire generations of students to emulate the subject it portrays.

THE GEORGE HERBERT JONES CHEMICAL LABORATORY OF THE UNIVERSITY OF CHICAGO

A LARGE group of distinguished chemists attended the dedication ceremonies, which extended over December 16 and 17. Mr. Jones, in presenting the laboratory, said: "The department of chemistry has done exceedingly well in the past; I hope that this new laboratory will assist it to accomplish much in research in the future, and especially will prove of value to industry."

President Robert Maynard Hutchins, in accepting the building for the university, said: "Through the generosity of Mr. Jones, a department with a brilliant history may look forward to a yet more brilliant future. Of the two hundred and sixty men and women who received the Ph.D. degree on recommendation of the department, twenty-four are heads of departments in colleges and universities; fifty-seven hold university professorships; fifty-eight occupy college chairs; twenty-two are active in research institutions, and eighty-five are engaged in industrial research. Obviously, what goes on in the department affects the progress of scientific investigation throughout the country, and has the most direct and important bearing on our educational system."

A plaster cast of the bronze bust of the donor, which when completed is to be placed in the lobby of the laboratory, was presented to the university by Mrs. Walter J. Jarratt, daughter of Mr. Jones, and accepted by Mr. David Evans, president of the Chicago Steel Foundries. Dr. F. W. Upson, a Ph.D. of the department, presented on behalf of alumni and friends a bronze bust of the late John U. Nef, and Mrs. Sara Bowles Smith presented a bust of the late Alexander Smith, both at one time members of the department.

Associate Professor John W. E. Glattfeld spoke on "Nef, the Man and Teacher," and Dr. Herman A. Spoehr, of the Carnegie Institution, Stanford University, on "Nef, the Investigator." W. D. Richardson, chief chemist of Swift and Company, gave an address on "Smith, the Man and Teacher," and R. H. McKee, professor of chemical engineering in Columbia University, spoke on "Smith, the Investigator."

A bust of Professor Stieglitz was presented by Dr. B. B. Freud, on behalf of alumni and friends, and accepted by Professor Hermann I. Schlesinger.

Participating in a program on the general subject of "Some Present and Future Problems in Chemistry" were Dr. G. N. Lewis, head of the department of chemistry in the University of California, whose topic was "Physical Chemistry"; Dr. C. H. MacDowell, president of the Armour Fertilizer Works, "Chemistry Applied to Industry"; Dr. Edward R. Weidlein, director of the Mellon Institute of Industrial Research in Pittsburgh; Dr. Charles A. Kraus, head of the department of chemistry in Brown University, who discussed "Inorganic Chemistry"; Dr. Moses Gomberg, head of the department of chemistry in the University of Michigan, and Associate Professor M. S. Kharasch, whose topic was "Organic Chemistry." Dr. Carl Voegtlin, of the United States Public Health Service, and Dr. A. P. Locke, Seymour Coman fellow in chemistry applied to medicine, spoke on "Chemistry and Medicine."

SCIENTIFIC NOTES AND NEWS

THE one thousand dollar prize offered annually by the American Association for the Advancement of Science for "a noteworthy contribution to science" presented at the meeting was awarded at Des Moines to Professor Arthur J. Dempster, of the University of Chicago. Professor Arthur H. Compton, with whom Professor Dempster is associated at Chicago, is reported to have said: "The most important contribution of twentieth-century physics is that the physical world can be reduced to three kinds of particles—protons, electrons and photons, and that each of these particles has also the characteristics of waves. The last stage is the proof that the protons, the positively charged parts of matter, have wave characteristics. It is this completion of the great work of twentieth-century physics which has been accomplished by Professor Dempster."

THE first gold medal awarded by the New York Academy of Medicine has been conferred on Dr. Carl Koller. The medal was presented by Dr. John A. Hartwell, president of the academy, the citation read by Dr. Linsly R. Williams being as follows: "Dr. Koller, distinguished ophthalmologist, able in the field of biology, painstaking in research, the discoverer of the anesthetic properties of cocaine, the inaugurator of the era of local anesthesia, conferring on humanity an enormous relief from suffering, a fellow of this academy since 1898, in recognition of his services is awarded this first academy medal."

THE Perkin medal is to be presented on January 10 to Dr. Herbert H. Dow, president of the Dow Chemical Company, of Midland, Michigan. The presentation is made because of his developments of improvements in the production of chlorine, bromine, magnesium and numerous other chemical materials. The presentation will be at a joint meeting of the Society of Chemical Industry, American Chemical Society, the Société de Chimie Industrielle and the American Electrochemical Society. An informal dinner at 7:00 P. M. will precede the meeting. The following program will be presented in Rumford Hall of the Chemists' Club at 8:15 P. M.: "Early Days of the Medalist," James T. Pardee; "Accomplishments of the Medalist," E. O. Barstow; presentation of Perkin medal, Marston T. Bogert, and acceptance speech, Herbert H. Dow. Dr. Dow will speak on the "Economic Trend in the Chemical Industry."

THE A. Cressy Morrison Prizes of the New York Academy of Sciences for 1929 have been awarded as follows: For the most acceptable paper in the field of experimental biology, embodying the results of original research not previously published, to Michael Heidelberger and Forrest E. Kendall, for their joint paper entitled "A Physico-chemical Interpretation of an Immune Reaction: A Quantitative Study of the Precipitin Reaction between Type III, Pneumococcus Polysaccharide and Purified Homologous Antibody." For the most acceptable paper in a field of science covered by the academy or an affiliated society, but not included in the above, which paper embodies the results of original research not previously published, to Horace W. Stunkard, for his paper entitled "The Life History of *Chryptocotyle lingua* (Creplin) with Notes on the Physiology of the Metacercariæ."

DR. THEOBALD SMITH, of the department of animal pathology of the Rockefeller Institute for Medical Research at Princeton, has agreed to act as consultant in bacteriology to the division of laboratories and research of the New York State Department of Health at Albany. Dr. James Ewing, professor of pathology at Cornell University Medical College in New York City, has accepted the appointment of consulting pathologist to the laboratories.

DR. HERBERT V. NEAL, professor of zoology and dean of the graduate school of Tufts College, was elected president of the American Society of Zoologists at the recent Des Moines meeting. E. E. Just, professor of zoology in Howard University, Washington, D. C., was elected vice-president.

AT the annual meeting of the Board of Trustees of the American School of Prehistoric Research held at Vassar College, December 28, 1929, Professor George Grant MacCurdy, of Yale University, was re-

elected director of the school for a term of two years. With the exception of 1922 and 1923, Dr. MacCurdy has been director of the school since it was founded nine years ago.

AT the annual meeting of the Royal College of Physicians, London, on December 5, Sir Norman Walker was elected president and Dr. Robert A. Fleming vice-president.

PROFESSOR THOMAS J. TALBERT, of the department of horticulture in the college of agriculture of the University of Missouri, has been elected governor of the Missouri-Kansas-Arkansas district of Kiwanis Clubs for the year 1930.

DR. HARVEY W. WILEY has resigned as director of the *Good Housekeeping* Bureau of Foods, Sanitation and Health which was established by and for him in 1912. He becomes director emeritus.

THE *Bulletin* of the American Mathematical Society reports that on account of the death of Dr. J. W. L. Glaisher, editor of the *Quarterly Journal of Mathematics* and the *Messenger of Mathematics*, the former journal will be discontinued, with the completion of its fiftieth volume. Professor G. H. Hardy has consented to edit the current volume (volume 58) of the *Messenger of Mathematics*, and it is hoped that this journal can be continued.

DR. HENRY KNUTE SVENSON has taken up the work of curator of plants at the Brooklyn Botanic Garden. Previous to accepting this appointment, Dr. Svenson was connected with the editorial office of *Biological Abstracts*.

E. P. HENDERSON, of the U. S. Geological Survey, has been appointed assistant curator of applied geology in the U. S. National Museum.

NELSON S. PERKINS has been appointed to succeed Dudley F. Holtman as construction engineer of the National Committee on Wood Utilization.

DR. ALBERT F. ZAHM, who was recently appointed to the new Guggenheim chair of aeronautics at the Library of Congress, has taken up his new work. He has been director of the aerodynamic laboratory of the Navy Department since 1916.

DR. SANFORD R. GIFFORD, of Omaha, who has been appointed head of the department of ophthalmology at the Northwestern University School of Medicine, Chicago, took up his work on January 1.

DR. L. A. CALKINS, formerly head of the department of obstetrics and gynecology at the University of Virginia, has been appointed head of the department of obstetrics and gynecology at the University of Kansas School of Medicine.

DR. WALTER B. CANNON, George Higginson professor of physiology at the Harvard Medical School, has sailed for France to serve as Harvard exchange professor at the Sorbonne and at the Ecole de Médecine in Paris. He will lecture also at provincial universities in France and in the Belgian universities during the second half year.

DR. FREDERICK L. HOFFMAN, consulting statistician, Wellesley Hills, Massachusetts, will participate in the Latin-American Medical Congress, reading an address on "The Value of Vital Statistics in Latin-American Countries." Dr. Hoffman will go by aeroplane from Birmingham to Mexico City, and will also make a flight to Yucatan. He expects to cover about three thousand miles or more by plane before he returns.

THE *Journal* of the Washington Academy of Sciences reports that Professor C. H. Ostenfeld, director of the Botanical Garden and Museum, Copenhagen, Denmark, recently spent several days in Washington, giving particular attention to Alaskan plants in the National Herbarium, especially those of Arctic Alaska. His studies were undertaken in connection with the preparation of a flora of northern Canada, a project upon which he is jointly engaged with Dr. M. O. Malte, chief botanist of the Canadian National Herbarium, Ottawa.

LLEWELLYN N. EDWARDS sailed from New York on December 20 for the British Isles, where he will study methods of design and construction of highways and bridges and will also devote some time to engineering research and investigation work in progress there. He plans to make a study of early bridge structures having historical value in the engineering profession.

DR. PAUL BARTSCH, of the U. S. National Museum, has returned from a trip to the West Indies, made under the auspices of the Walter Rathbone Bacon traveling scholarship. He visited most of the islands between Porto Rico and Trinidad, except Antigua and Barbuda, which had been thoroughly explored by Mr. J. B. Henderson, and Barbados. After leaving Trinidad, his expedition sailed along the coast of South America, visiting the Leeward Islands, Margarita, Orchilla, El Roque, Bonaire, Curaçao and Aruba.

DR. ALAN M. CHESNEY, associate professor of medicine of the Johns Hopkins School of Medicine, will deliver the fourth Harvey Society Lecture at the New York Academy of Medicine, on Thursday evening, January 16, 1930. His subject will be "Acquired Immunity in Syphilis."

DR. HENRY A. CHRISTIAN, Hersey professor of the theory and practice of physic at the Harvard University Medical School, will deliver the annual lecture

of the Scripps Metabolic Clinic at La Jolla on January 25 on "Chronic Non-valvular Heart Disease: Its Causes, Diagnosis and Management."

THE address of the retiring president of the Philosophical Society of Washington was given on January 4 by Dr. Leason H. Adams on "The Significance of Pressure in Geophysical Investigations."

THE biennial Huxley lecture was given by Sir William Bragg in the Charing Cross Hospital Medical School on November 28 on "The Crystal Structure of Organic Substances in Its Relation to Medicine."

BOTANISTS, plant physiologists and phytopathologists planning to visit France at the time of the botanical meetings to be held in Cambridge next year are asked to communicate as soon as possible with Dr. Jean Dufrénoy, Station de Pathologie Végétale, Etoile de Choisy, Versailles, S. O., France, in order that an organized trip through France may be arranged.

AN Associated Press dispatch reports that seventeen Russian agricultural engineers arrived from Moscow on December 29 on the *Nieuw Amsterdam* to study the manufacture of farming implements in the United States. They are reported to have said that the Soviet government is planning to construct the world's largest factory for the manufacture of agricultural machinery and implements at Nijni-Novgorod.

THE twentieth annual exhibition of electrical, optical and other physical apparatus was held by the Physical Society and the Optical Society, London, on January 7, 8 and 9, at the Imperial College of Science and Technology, South Kensington. As on previous occasions, there was a trade section and a research and experimental section.

THE yacht *Corsair* has been formally transferred to the service of the United States by Mr. J. Pierpont Morgan. The yacht was renamed the *Oceanographer* before the transfer was made and will be used in the recharting of the Atlantic and Gulf seaboard by the Coast and Geodetic Survey. It is stipulated that the vessel shall not be disposed of by the government until it is scrapped. It will be placed under the command of Captain F. L. Peacock, of the Coast and Geodetic Survey.

THE Secretary of the Interior announced on December 2 that the advisory committee on education by radio had received grants from the Carnegie Corporation of New York, the J. C. Penney Foundation and the Payne Fund, amounting to $16,500, and that the Payne Fund had lent the services of an expert for several weeks. With these resources it is expected that the committee will continue to conduct investigations into the extent to which radio has been used in school work and by colleges for broadcasting programs of an educational nature. The committee intends to make some recommendations for scientific research designed to ascertain the effectiveness of radio as an educational tool.

EMILE PICARD, general secretary of the Academy of Sciences in Paris, has received from an American, who prefers to remain anonymous, a check for 300,000 francs, for the "Amis des sciences," a society organized for the aid of unfortunate scientific men and their families.

THE Field Museum of Natural History has begun publication of a monthly bulletin, *Field Museum News*, which is to be circulated among the institution's members, now numbering nearly 6,000. Announcements, reports and records of all museum activities will be published in the periodical, including notes on additions and improvements in the exhibition halls, and accounts of the work conducted in the museum's scientific research laboratories, and by its many expeditions operating in widely scattered and remote parts of the world. Dr. Stephen C. Simms, director of the museum, is the editor. Contributing editors include Dr. Berthold Laufer, curator of anthropology; Dr. B. E. Dahlgren, acting curator of botany; Dr. O. C. Farrington, curator of geology, and Dr. Wilfred H. Osgood, curator of zoology. H. B. Harte, head of the museum's division of public relations, is managing editor. The paper is printed by Field Museum Press, the museum's own plant, which also prints the scientific books and pamphlets published by the museum. A feature of the January issue is the first installment of a history of the museum since it was founded by Marshall Field in 1893, written by Dr. Farrington, who is dean of the scientific staff, having been head of the department of geology since the museum's earliest days.

THE mining interests of central Pennsylvania have organized an advisory board to cooperate with the school of mineral industries of the Pennsylvania State College. At a recent meeting in Johnstown members of the board studied the curriculum and research projects with the object of making any suggestions which might bring the college service closer to the industry. A board representing the western mining interests was organized in December and one from the anthracite fields will be formed in the spring.

Industrial and Engineering Chemistry reports that efforts of American chemists to save from destruction by mold the only official records of the construction period of the Panama Canal, a series of five mural paintings in the Administration Building at Balboa, have been successful. W. B. Van Ingen, instructor in Cooper Union, supervised the treatment of the pictures. When the presence of mold, the gangrene of paintings, became apparent on the pictures last year, Mr. Van Ingen, who painted the series at the request of George R. Goethals, builder of the canal, was com-

missioned by Harry Burgess, governor of the Canal Zone, to undertake the restoration of his paintings. The treatment finally used was devised by Albert B. Newman, director of the department of chemical engineering at Cooper Union, with the cooperation of Charles Thom, chief mycologist of the Bureau of Chemistry and Soils of the Department of Agriculture; Alexander Scott, of the British Museum, and other mold experts. Mr. Van Ingen said that the thin film of paraffin covering his pictures would remain unchanged indefinitely, and would prove an absolute protection against the effects of exposure to the tropical climate as long as it existed. The use of paraffin was suggested by the experience of New York City engineers, who made an extensive examination of the Obelisk in Central Park to determine how it could be protected from the city climate. Among the mold experts who assisted Mr. Van Ingen and Professor Newman, in addition to Drs. Thom and Scott, were: Lewis T. Bates, chief of the laboratories of the Health Department of Panama; Bernard O. Dodge, plant pathologist of the New York Botanical Garden, where important research in this field is in progress; Charles F. McCoombs, of the New York Public Library; T. R. Beaufort, professional picture restorer; Leslie Ewart Morris, of the British Cotton Research Association; George Smith, of England, and Hugh L. Robinson, editor of the *Journal of the Textile Institute of Manchester*.

Plans are now well advanced, under the direction of Sir Richard Allison, chief architect to the Office of Works, London, for the construction of the building which is to house the Museum of Practical Geology. The museum is at present in Jermyn Street, and the new site is in South Kensington, adjoining the Science Museum in Exhibition Road, just beyond the northeast corner of the Natural History Museum, and the proposal is to connect the new structure by a bridge with the Science Museum, from which it will be only a few yards distant. Although the plans for the new building are not yet complete, they are far enough advanced for a beginning to be made very shortly in constructing the foundations. The estimated cost of the new building is £220,000. The general scheme is that it shall be on four floors, with a top-lighted roof and central well. The problem of finance is simplified by the fact that the site in Jermyn Street, which belongs to the Crown, is of great value. The museum was founded in 1852. Its library is essentially a library of the geological surveys and maps of the world, together with the economic geology of Britain, the British Dominions and the world generally. Huxley and Tyndall were among those who at one time lectured in the museum. In 1900, during reconstruction, the lecture hall was thrown into the museum library. It is hoped to resume the lectures in the new building or in the lecture theaters of adjacent museums in South Kensington.

The British Medical Journal reports that the act to incorporate a Royal College of Physicians and Surgeons of Canada received last June the royal assent, and in accordance therewith an inaugural meeting, convened by the general secretary of the Canadian Medical Association, was held at Ottawa on November 20. This meeting was attended by sixty of the leaders of the medical profession in Canada, designated charter fellows, and it marked the culmination of a movement which began in 1920. The act provides that the council may organize the college into medical and surgical divisions, those admitted into them being designated fellows of the Royal College of Physicians of Canada or fellows of the Royal College of Surgeons of Canada (or their equivalents in the French language). At the inaugural meeting, Toronto was chosen as the headquarters of the college; Professor Jonathan C. Meakins, of McGill University, Montreal, was elected president; Professor Duncan Graham, of Toronto, vice-president in charge of the medical division, and Dr. F. N. G. Starr, also of Toronto, vice-president in charge of the surgical division. Dr. T. C. Routley, general secretary of the Canadian Medical Association, was elected registrar-secretary.

DISCUSSION

SOME LIMITATIONS OF THE EXPERIMENTAL METHOD IN BIOLOGY

At the present time much is being written as to the value of the experimental method of attack in biological problems. There can be little doubt of the great utility of the experimental method of approach, but in itself this method may lead to extremely fallacious conclusions. A notable example, apparently, of this deplorable result is presented by current investigations on the experimental production of new species.

Obviously, in pursuing the study of experimental evolution we must keep our eye closely focused on the conditions in nature because it is under the conditions prevailing in nature that new species appear from time to time. The normal course of events in the case of the coming into existence of members of a species has been contemptuously designated by one of our prominent experimental biologists as "the passing show." If we were to continue the figure of the show, we would on the same basis naturally characterize a

great deal of the experimental work on species, at the present time, as a "side show."

One must, indeed, apparently regard the prevailing use of radiations on the chromosomes of plants and animals in this light. It has been shown beyond any question that it is possible to modify the chromosomal structure in both plants and animals by means of radiations of various sorts. It has not been shown that such treatment can produce permanent and normal species. Further, it is far from clear that radium emanations or X-rays can ever have played any important rôle in affecting permanently the chromosomal organization of the germ-cells in either plants or animals. Consequently, from the practical standpoint of the experimental evolutionist, actinic experimentation is not likely to lead to any result of lasting importance. In making this statement it will naturally be admitted that such experimental work is of great interest and, from the standpoint of the pathologist and the surgeon, may be ultimately of great importance. From the standpoint of the investigator of the origin of species, it is ruled out of court at the outset by reason of the non-viability of its product.

We are becoming more and more aware, particularly on the plant side, that species cross in nature to a hitherto unexpected extent, and that when two species are crossed almost anything may happen, from the production of an almost infinitely variable progeny to the origin at once of a new, fixed species. It is apparent as a result of correlated cytological and experimental work that many of our large genera of plants are composed of species which are more or less contaminated by hybridism. This, for example, is notably the case with the roses of Europe, the hawthorns of America and the blackberries of both continents. Nor is the northern hemisphere alone in this hybrid multiplication of species. The hundreds of species of Acacias, or "wattles," in Australia afford a similar case of multiplication and variability of species due to hybridization. The same is true of the Australasian genus Eucalyptus of which there are so many and so variable species that their study has given rise to specialists in Eucalyptology.

In his "Origin of Species," Darwin, with that sanity of judgment which makes him so outstanding a figure in the biological sciences, pointed out that the most important element in the origin of species was the innate tendency of species to vary. In his time no explanation was forthcoming of this tendency to variability, but Darwin, with characteristic keenness of insight, pointed out that the greatest variability was found in the species of large genera. This statement, in view of the investigations of the last two decades, was nothing less than prophetic. As a consequence of our present correlation of experimental and morphological study of natural and experimental hybrids, we have come to realize that many species of plants in nature are the result of previous hybridization. There can be little doubt that the same condition prevails on the zoological side and under the same conditions. It is becoming, for example, apparent that many organisms representing large genera of animals are extremely variable and in many instances present the cytological characteristics of hybrids.

One of the most fruitful fields of investigation in connection with Darwin's great work, "Origin of Species," was the comparison between domesticated animals and cultivated plants and plants and animals in nature. In our time, hybridism plays an enormous rôle among the plants of our gardens. Many of the most desirable of these are known to be of hybrid origin. Further, many of our useful plants whose origin is buried in antiquity, by their variability and cytological peculiarities, seem clearly to represent old-time hybrids. This is notably the case, for example, in such common plants as the potato, Indian corn, etc.

Not only are many cultivated plants of known or suspected hybrid origin, but more and more examples of hybridism are being recognized in nature. For example, Trelease has called attention to the fact that there are practically as many hybrids of our American oaks as there are recognized species. A similar situation has been emphasized by Kerner in the case of European willows. Brainerd has described a large number of hybrid violets, and has resynthesized them by crossing again their supposed parents. Almost numberless examples have been supplied in recent years of natural hybrids of plants in all parts of the world and under all conditions. In animals the situation is as yet not so clear, but hybridism is the most natural explanation of the great variability and numerous species of air-breathing snails, of Orthoptera, Lepidoptera, etc., etc.

A particular case of variability is presented by so-called mutations. Plants and animals presenting this sort of variation which leads to the immediate formation of relatively fixed types (elementary species), present striking features of resemblance to known hybrids, particularly in the meiotic or reduction division. Further, in Leptinotarsa (the potato-beetle), among animals, and in Rubus and in the Brassica-Raphanus cross among plants, known hybridization has been followed by clear mutation.

The unwillingness of certain geneticists to admit the hybrid origin of mutating types is one of the outstanding features of present activity in evolutionary investigation. It is probable that this unwillingness arises out of the fear that the Mendelian hypothesis or the chromosomal hypothesis of heredity would be more or less compromised by such an admission. Cer-

tainly hybridism furnishes a satisfactory and sufficient explanation of mutation.

As regards the experimental production of species at the present time, we must choose between the practicality of the experimental production of new species by the action of radiations and by that of crossing. It seems scarcely possible to question the choice which should be made. A very weak point in connection with the production of species by radiations is the fact that there is no good reason to suppose that radiations of sufficient power have been active in this manner to any important extent in nature. Secondly, if such radiations were active, there is very grave doubt, on the basis of existing experimental work, whether they could produce viable species. If we take the other alternative, that of hybridization, we find an overwhelming amount of evidence for its occurrence in nature in all parts of the world and under all climatic conditions. Common sense would seem to indicate that the study of hybrids is much more likely to lead to permanently valuable results in experimental evolution than that of the effect of radiations.

The former generation attributed marvelous powers to electricity, which was then coming into the foreground in practical relation to human affairs. Almost every household attempted to cure itself of the various ills of humanity by the use of electrical appliances, from batteries to electrical belts. At the present time radiotherapy is as prevalent and as popular in the public mind as electrotherapy was a generation or two ago. An outstanding feature of our time is ray-mindedness. The popular exponents of biology have merely found this ray-mindedness an easy line of exploitation. There is no reason to believe, however, that radiations will, in the long run, occupy a higher position in biology, or even the popular understanding of that subject, than does electricity. We must apparently come back, as a consequence, to the sound Darwinian conclusion that environment may select variations but it can not in any important respect give rise to them. This dictum is apparently quite as true for special chemical and physical conditions as it is for the general non-living environment.

EDWARD C. JEFFREY

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ADSORPTION AND EMULSION FORMATION

IF oil and water be shaken vigorously together in a bottle or test-tube, an emulsion is formed which soon separates completely on standing. But if oil and mud be shaken together, the emulsion formed is a thick paste that not only does not disintegrate on standing but resists heating and chemical reagents to a remarkable degree. The oil used may be crude or refined, heavy oil, gasoline or even ether. The mud used may likewise be almost anything: sand, clay, road dirt, metal powder or fibrous dust mixed with water. The mud may be acid or alkaline. Gasoline and clay mud make a convenient pair to experiment with.

We get a clearer picture of what the soil, water and solid particles are doing if we take merely muddy water and shake it up with gasoline. The oil and water now separate clear quite quickly, leaving the solid particles in the form of a thin skin at the interface and on the glass wall. It is quite startling to see a clay suspension, that would not settle clear in months, changed to clear water in a few minutes by shaking with gasoline. The skin formed by the fine particles is quite tough and may be lifted out on a wire like a piece of wet rag. On drying it returns to a powder, indicating that no permanent chemical change has taken place. The solid particles can not spread (like a liquid) at an interface, and being themselves insoluble, can only adsorb the two liquids and mesh together.

Reinders[1] in 1913 made many observations with powdered metals and insoluble salts in water shaken with ether, kerosene, the alcohols, etc., and stated the theory very clearly—the interfacial tension between the two liquids must exceed the sum of the other two. Hofmann[2] about the same time made an extended series of similar observations. Wheeler P. Davey[3] in 1926 read a paper before the Fourth Colloid Symposium giving the theory of the formation of cup grease, which is a carefully prepared emulsion of water, soap and heavy oil. Davey's idea is that the long soap molecules (sodium stearate) in such an emulsion all have their small OH ends turned inward toward the water droplets and their larger CH_3 ends turned outward toward the oil. The meshing together of these bristling units (like hair brushes or chestnut burs) makes a stiff paste of the cup grease. Heating to 250° C. completes the dispersion and removes excess moisture.

The theory of adsorbed polar molecules is hardly sufficient to account for the formation of a tough layer of solid particles at an interface between two liquids. Each particle may be an aggregate of tens of thousands of molecules each but weakly polar in itself. Very clean dry sand easily floats on water, the grains gathering in patches. Here the lighter upper fluid (air) is adsorbed on all sides of the sand grains, causing them to float, and the air layers on adjacent grains tend to coalesce, bonding the grains together. However, in the clay-water-gasoline emulsion, the fine particles of silicates constituting the clay are them-

[1] Reinders, *Kolloid Zeitschrift*, 13: 235, 1913.
[2] Hofmann, *Zeit. Physik. Chemie*, 83: 385, 1913.
[3] Davey, Fourth Colloid Symposium Monograph, p. 38.

selves hydrophilic, hence the assumption of an oil film completely enclosing each grain is untenable.

If a mass of clay-water-oil paste be left floating on a soda (washing soda) solution for some time, the silicate particles gradually free themselves from the emulsion and sink. But each drags down with it a tiny droplet of oil, just easily visible with a 10× pocket magnifier, attached to its top. The soda has completely freed the surface of each grain from oil but has *not* destroyed the attraction of the solid particle for the oil droplet. Such behavior is not readily accounted for by either the mechanical or chemical theories of adsorption. An electrical theory would account for it if a particle of one dielectric partly immersed in another had an opposite charge induced on the remaining part. I find no reference to any such law in the literature, but it is plainly consistent with the current theory of dielectrics.

In the oil fields conditions are frequently ideal for emulsion formation. During drilling, oil and mud are intimately mixed by the drill bit. During flowing and pumping, flow through the fine capillaries of the oil sands will produce emulsions if free sand particles and water are present. Such emulsions in most cases represent a dead loss, for there is no efficient chemical method of separating the oil at a reasonable expense. The high voltage electrolytic method (Cottrell process) effectively separates the particles constituting the emulsion but requires expensive apparatus.

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EXCYSTATION OF COCCIDIAL OOCYSTS IN VIVO

WHILE excystation of coccidial oocysts has been observed and deliberately produced *in vivo* by various observers the phenomenon has not been put to the practical use of which it is capable. Segmentation of the oocysts is the usual method of determining the viability of coccidial oocysts, but the limitations of this method lie in the fact that only unsegmented oocysts may be thus tested. Excystation is a reliable criterion of viability that can be used to determine the length of life of oocysts after segmentation, the action of physical or chemical changes in the environment of segmented oocysts or of any other experimental procedure which can be tested by a conclusive manifestation of life within the matured oocyst.

The author has repeatedly carried out excystation *in vivo* using the following simple technique. Segmented oocysts from cats, dogs, guinea-pigs, pigs and prairie-dogs have been used with equal success; probably any species of coccidia from birds or mammals can be used. Young rats (75 to 100 grams) are deprived of food and water for twenty-four hours preceding the experiment. This has the double advantage of making the animal eager to eat and of thoroughly emptying the stomach and small intestine. The ripe oocysts are concentrated by centrifugation. If they have been exposed to any unpalatable chemical which may have been used to prevent putrefaction or for some experimental purpose, the chemical must be removed by dilution with water and centrifugation. The concentrated oocysts are suspended in four or five drops of sweet milk. The material is offered to the starved rat and will be immediately consumed. Sixty minutes after the ingestion of the oocysts, the rat is killed and the intestine is removed. At various points throughout its length it will be observed that the small intestine is distended with white contents. At these points the intestine should be opened and the contents observed microscopically. By examining various places in the intestine, all stages of excystation, if the ingested oocysts were normal, may be found, including motile sporozoites within and outside of the oocysts.

When using this method for experimental purposes, one or more control animals fed with untreated oocysts of the same lot from which the experimental were obtained should be included in the experiment.

JUSTIN ANDREWS

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INVESTIGATIONS OF APPLICATIONS OF IODINE

FOR some time past, especially in Europe, considerable research attention has been accorded to the investigation of various proposed uses of iodine. This element, while less fortunate commercially, perhaps, than its congener bromine, which among other industrial applications has achieved importance in the manufacture of a widely used "anti-knock" motor fuel, is known to play a vital rôle in physiology, which fact may lead, it is thought, to the extension of its utility in food and medicine as well as in agriculture. Through the brilliant investigations of Kendall, Harington, McClendon and others, the intricate biochemistry of iodine has received much illumination. There remain, however, many unsolved questions regarding its physiological functions, especially in the lower animals.

These reasons led the Iodine Educational Bureau to establish on January 1, 1928, a multiple fellowship at Mellon Institute. This fellowship, having as its objects the investigation of possible technologic uses of iodine and also collaboration with other institutions interested in research on this element, is headed by

Dr. George M. Karns, who was formerly a member of the chemical faculty at the University of Illinois. All experimental findings of the work will be made available for general use through the scientific journals.

A recent appropriation from the Iodine Educational Bureau has enabled the fellowship to expand its activities by arranging for the study at the Pennsylvania State College of the nutritional functions and value of iodine in the feeding of live-stock. This comprehensive project, begun on September 26, 1929, under the direction of Professor E. B. Forbes, of the Institute of Animal Nutrition, will include studies on cattle, sheep and swine. Such information is much needed, because most of the work on the part played by iodine in metabolism, especially with reference to the thyroid, has been confined to man. Dr. Karns and his associates at Mellon Institute are cooperating in this research, chiefly by preparing standardized feeds.

The institute, acting for the iodine fellowship, has also made arrangements for another investigation under the supervision of Dean Charles H. LaWall, of the Philadelphia College of Pharmacy and Science. A scholarship, which will be held by Mr. L. F. Tice during the college year 1929–30, has been established at that institution for the purpose of making a broad study of vehicles and solvents for iodine with the view of evolving a more satisfactory preparation for medical use than the tincture now employed. A number of new organic chemicals will be studied according to a definite program which has been worked out.

Mellon Institute is also considering, with the advisory aid of a number of pharmacologists, the founding of a medical research scholarship for the purpose of aiding in the solution of questions regarding the utility of iodine in internal medicine. A foundation of this type would, of course, be made in some institution possessing special facilities for such research.

The results of all work, both of the iodine fellowship at Mellon Institute and of the scholarships founded at other institutions, will be made available to the public through scientific periodicals. This procedure is in harmony with the Iodine Educational Bureau's general policy of releasing the findings of all investigations made under its auspices.

Lawrence W. Bass

Mellon Institute of
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WHAT IS THE BEST SYSTEM OF PRESENTING BIBLIOGRAPHIES?

In the issue of Science for August 30, 1929, Dr. J. L. St. John very appropriately calls attention to the desirability of a more uniform system of presenting bibliographies in the various scientific journals. With reference to the name-number and name-date systems of citing literature, however, he says "It would seem that the advantages of the name-date system justify its use in practically all cases" and "A number associated with the author's name has no value except in helping locate the reference in the bibliography." In view of these statements it seems desirable to call attention to the other side of the question. As there are journals, editors and writers who prefer the name-number system there must be good reasons for using it.

A working corollary of the name-number or numerical system is that the list of literature citations is arranged alphabetically. This arrangement, however, is only an additional advantage of the system and can be disregarded if it is preferred that the numbers be assigned to references in the order in which the references appear in the text or in chronological order or in the order of importance or in any other order determined upon. Aside from the convenience attaching to the use of numbers in the list of references, the major advantage is derived from their use in the text.

To give one of many examples of this advantage, I quote from a comparatively recent publication: "This is fully covered in the literature (2, 8, 14, 17, 18, 20, 22, 25, 29, 32, 41, 59, 60, 61, 64, 66, 68, 83, 85)." If these citations had been given according to the name-date system the following would have been the printed result: "This is fully covered in the literature (Anonymous [1903]; Atlantic City Academy of Medicine [1902]; Banks [1927]; Broadbent [1895]; Brooks [1916]; Bulstrode [the date of publication was 1904, but the report covered in this instance was for 1902–03]; Bundesen [1925]; Chantemesse [1896]; Conn [1895]; Eade [1895]; Harris [1925]; Lankester [1885]; Lumsden, Hasseltine, Leake and Veldee [1925, but the report was for the year 1924–25]; Marvel [1903]; Mosny [1899]; Newsholme [1903]; Pease [1911]; Stiles [1912]; Thresh and Wood [1902])."

In addition to the increased volume of text and expense of publication there is the greater liability of citation errors when so many names and dates are included. Not infrequently an author gives group citations many times in his paper. The page references are sometimes included in the group citations, and there may also be several citations of the same year or of different years from the same author. In the latter case it is necessary to differentiate between the citations of the same year from one author by the addition of the letters a, b, c, etc., thus further complicating the situation. Another objection to the name-date system occurs in connection with the cita-

tions of the proceedings of some society whose meetings are held one year but whose publication is not accomplished until the following year. Furthermore, some bound volumes of periodicals cover publications of two years, or the last half of one year and the first half of the succeeding one. Accuracy and clearness become serious problems in the textual citing of such references according to the name-date system. In comparison with that system the numerical system has the advantage of being concise, accurate, definite and less expensive, and the numbers are easily inserted, inconspicuous, and their use in a small space does not distract the reader's attention from the subject-matter of the text. These advantages become especially evident in a paper containing many references to the same citation. Finally, the use of the numerical system permits the inclusion in the text of whatever names and dates are required.

An objection that is sometimes raised against the numerical system is that after a manuscript has been prepared, particularly when it reaches the galley-proof stage, the author finds another reference which he very much desires to add. If the name of the author of the new reference happens to begin with W or Y there may not be much difficulty in making the insertion, but if it unfortunately begins with A or B there may be trouble, especially if the list is long. An easy solution for this difficulty is to designate the new reference by a numbered letter. Thus if the previous citation in the alphabetical list is number 3, the inserted one may become 3a without any serious disturbance. If the list is not arranged alphabetically, and of course that arrangement is not a positive necessity when numbers are used, the insertion of new references could easily be made at the end of the list and numbers assigned accordingly.

M. C. MERRILL

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SCIENTIFIC BOOKS

The Platinum Deposits and Mines of South Africa. By DR. PERCY A. WAGNER. Oliver and Boyd, Edinburgh. 38 full-page plates, 3 maps, 37 text illustrations. 326 pp. Price 21s.

WHILE this volume discusses primarily the deposits of the Transvaal, it also includes those of the Cape Province and southern Rhodesia, which are closely related geologically with those of the Transvaal. The South African deposits have been much in the limelight since their discovery about four years ago, and this detailed exposition of the local situation by one so thoroughly versed in it will be welcomed by all interested in the subject. The story as related by Dr. Wagner is a marked tribute to Dr. Hans Merensky for the ability he displayed in opening up the new field, for he not only discovered both the dunite deposits and the Merensky Horizon in the Lydenburg district, as well as the more important locations in the Potgietersrust area, but also succeeded in locating the Merensky Horizon in the Rustenburg district after others had abandoned all hope of finding it.

It is with keenest regret that I have learned of the death of Dr. Percy A. Wagner, who was stricken with typhoid fever and died on November 11, 1929. He was one of the most talented men who has ever visited the great southern part of South Africa, and had just entered into a contract for five years to do mineral surveying, after having been director of the survey and doing much original work.

Dr. Wagner first discusses the sources of supply, the properties and uses, and the range of prices of the platinum metals. Several years ago some of the gold mines of the Rand developed methods of saving the small amounts of osmiridium that occur in these ores, and soon brought the union into the position of leading producer of this rare combination of metals. A few years later, the platinum deposits were discovered, and since the beginning of the industry in 1925 South Africa has reached third place as a producer of platinum group metals, being exceeded only by Russia and Colombia. For a number of years Canada has been producing a gradually increasing amount of the platinum metals as a by-product in the treatment of nickel ores, but yielded third place to South Africa in 1927.

Before the discoveries in South Africa, the occurrence of platinum was considered to be standardized as an alluvial product. While some was obtained as a by-product in the treatment of the ores metals, particularly nickel and copper, none was regularly obtained from ore mined *in situ* for the sake of its platinum content. As found in nature, the metal was seldom in the pure state, but usually occurred as an alloy of platinum with other metals of the platinum group and with iron; sometimes these were accompanied by gold, copper, nickel or cobalt. With the exception of these native alloys, only two minerals of the platinum metals were known: sperrylite, the arsenide of platinum, and laurite, the sulphide of ruthenium and osmium, and both of these were very rare. The former was found in the Canadian nickel ores and the latter in the platinum-bearing gravels of Borneo. In South Africa, then, for the first time, ores were found *in situ* in which platinum was found

in sufficient quantity to justify treatment for the recovery of the platinum content. These ores have not only given us our first extensive treatment of lode ores for platinum but have also contributed several items of more than ordinary interest in the mineralogical history of the metal. In addition to the native alloy minerals present in the ores, they contain platinum and palladium in a colloidal state, as well as various compound minerals. These ores have not only yielded single crystals of sperrylite up to 33.75 grams in weight (several hundred times larger than any previously known), but have also given us two new minerals: cooperite, a sulph-arsenide of platinum, Pt $(As, S)_2$, and stibiopalladinite, a palladium antimonide, Pd_3Sb. The ores of the Potgietersrust district are particularly rich in these compound minerals, while those from Lydenburg and Waterberg show mainly the alloy minerals. The presence of these colloidal metals and new minerals introduced new and difficult problems into the ore dressing and metallurgical treatment of the ores and are largely responsible for the delay that has been encountered in the development of the industry.

These South African platinum deposits present a unique chapter in economic geology, from every angle from which they may be viewed: chemically, mineralogically, geologically and geographically. The metals are found in both the free and the combined form and in association with both acid and basic rocks; they are found in all types of deposits: clays, sands, river gravels, marine beds, sandstones, quartzites, conglomerates and a variety of others; they are found in rocks of all ages, and in distribution they occur in a well-defined area reaching from the Insizwa Range in eastern Cape Province, north and east through the Black Reef, the Witwatersrand gold fields, the norite zone of the Bushveld Igneous Complex and the Waterberg district of the Transvaal, and on throughout the length of the Great Dyke of Southern Rhodesia, a total distance of about one thousand miles, giving what the author designates as "a great *platinum belt* that cuts indiscriminately across the oldest and youngest geological formations, completely disregarding geological structures and structure lines in the more superficial parts of the earth's crust."

The wide-spread occurrence of the platinum metals under such a variety of conditions is clearly a result of an underlying cause that has continued to operate throughout the various stages of geological time. According to Suess, the *sima* zone, a thick peridotite shell underlying the upper layers of the continental crust, is the principal home of the platinum metals. This would indicate that the *sima* zone underlying this portion of South Africa is abnormally rich in platinum which has been transferred to the surface layers of the crust. According to Spurr's conception of "ore canals" there should be here a great platinum canal that from earliest times has facilitated the upward transference of the platinum metals from the *sima* zone to the rocks of the overlying crust. Dr. Wagner predicts further discoveries of platinum along the course of this canal.

In the discussion of the less important sections of the platinum-bearing zone, three pages are devoted to the ultrabasic and basic rocks of the Swaziland system, nine pages to the Black Reef and the Rand, six pages to the Great Dyke, eleven pages to other minor sections and three pages to the eluvial and alluvial deposits; the bulk of the discussion, however, (165 pages) is concerned with the details of the deposits of the Bushveld Igneous Complex, associated with dunite, olivine, olivine-dunite, chromitite, magmatic nickel-copper-iron sulphides and particularly with the norites of the Merensky Horizon.

From a purely scientific standpoint, one of the most interesting sections of the book is the chapter contributed by Dr. H. Schneiderhöhn, of Freiberg, on the mineragraphy, spectrography and genesis of the Bushveld ores. This includes a study of eleven specimens from the Rustenburg district, seventeen specimens from Lydenburg and sixty-five specimens from Potgietersrust; mineragraphical observations on the occurrence of the platinum metals in the nickel-pyrrhotite-bearing rocks of the Bushveld Complex; a detailed description of the mineragraphy of sperrylite and the two new minerals, cooperite and stibiopalladinite; an account of the examination of various specimens for the platinum metals by the quartz spectrograph and ultra-violet light, and a summary of results and conclusions as to the genesis of the ores.

According to Dr. Schneiderhöhn these ores have passed through four phases in their formation: first, the earliest crystallization from the magma of such minerals as chromite, magnetite, etc.; second, the separation of the liquid sulphide melt from the silicate-oxide melt, and the crystallization of the sulphides; third, the crystallization of the residual solutions, and fourth, liquid exsolution-segregation and pegmatite formation. During the second of these stages the platinum metals entered into the crystal lattices of the iron-nickel sulphides and crystallized with them; during the third stage the temperature was too low to permit this, and the platinum metals were forced to crystallize separately, as sperrylite, cooperite and stibiopalladinite.

The problems involved in the mining of these deposits were soon solved, but the difficulties in ore dressing and metallurgical treatment were not so readily disposed of, and it is only within the last

year that really satisfactory methods have been developed.

In the months immediately following the discovery of the scope of the South African deposits, it was freely predicted that within three years the union would be the leading platinum producer of the world. That this has not been more nearly realized is due chiefly to the unprecedented difficulties encountered in the treatment of these new ores. Decided progress has been made, however, particularly within the past year, and another year should show pretty definitely what is to be the status of South Africa in the world's platinum industry. In 1928 the production was 23,600 ounces of platinum metals from three treatment plants totaling 300 tons of ore per day; three more are under construction with a daily capacity of about 800 tons of ore, and this should provide a potential capacity of more than 100,000 ounces of platinum metals a year.

It still remains to be seen how much South African platinum the world can absorb, and at what prices. To secure a market for anything like the maximum capacity will mean strenuous competition against Russia and Colombia, and from increasing amounts of by-product metals from Canadian nickel ores. To accomplish this through a price war would mean the sacrifice of much or all of the profits of all producers for several years to come. The only alternative is international cooperative restriction of output at a stabilized price, and there seems little prospect of accomplishing this at the present time. It is possible, however, that the country that produced a diamond syndicate that has successfully maintained its existence for thirty years may find a solution of this similar, but possibly more difficult, problem.

One of the chief requirements for the future success of the industry is an increase in the present rather limited demand, by the development of new uses. Russia already has a platinum institute for the fostering of her industry, and Dr. Wagner advocates the appointment of a committee of scientific and commercial men for the same purpose in South Africa, the necessary funds to be supplied by the government and the mining companies controlling the industry.

The volume is concluded by an excellent bibliography of platinum covering the last twenty years. The entire volume is well printed, and is illustrated by thirty-seven figures in the text, and thirty-eight plates, three of which are geological maps of the Bushveld, Rustenburg and Potgietersrust districts. Typographical and other errors in the text might almost be said to be conspicuous by their absence.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

AN ELECTRIC KYMOGRAPH[1]

THE aluminum box shown in Fig. 1 contains four shafts. Shaft No. 1 is at the back of the box and extends only to the block opposite the disk on which the drum will rest. Shaft No. 2 is at the front and extends from the partition enclosing the motor all the way to the right-hand wall of the box. Shaft No. 3 is on the right-hand side at the back of the box, and shaft No. 4 is just in front of No. 3.

Above the shafts are seen two levers, pivoted at the back. The left-hand lever may readily be moved into any one of six slots on the middle portion of the front wall of the box, and the right-hand lever may as readily be placed in any one of three slots on the right-hand portion of the front wall. Each slot is double—a front and a back slot. And each slot is undercut on the right-hand side. In Fig. 1, the left-hand lever just mentioned lies free in the open part

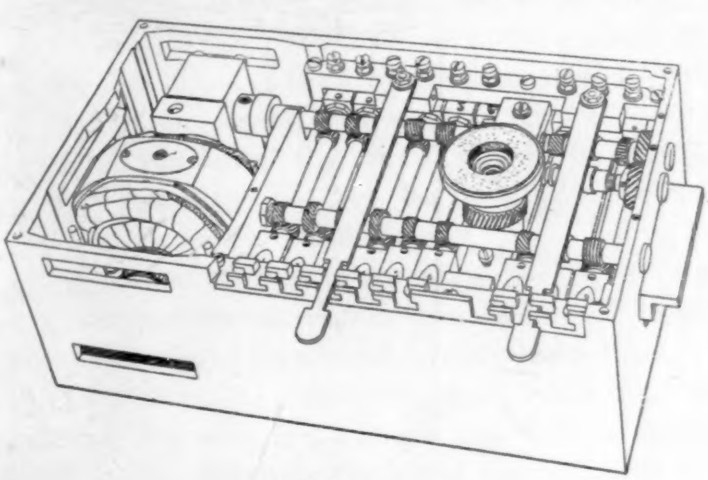

FIG. 1

of slot 4, whereas the right-hand lever is engaged beneath the overhang left by undercutting the middle one of its own group—the three right-hand slots. The left-hand lever in Fig. 1 can be lifted out of its slot directly, but the right-hand lever in Fig. 1 fits snugly in the undercut and must be moved to the left in order to free it. Obviously, the left-hand lever can be engaged in any one of its six slots and the right-hand lever can be engaged in any one of its three slots at the operator's will. Indeed, these levers may be moved from one slot to another in one or two seconds.

[1] This new kymograph was first given "public use" by Professor Charles W. Greene in his laboratory at the University of Missouri, Columbia, Missouri, January, 1929. The kymograph was formally demonstrated to the members of the Thirteenth International Physiological Congress in Boston, Wednesday, August 21, 1929. It will be found on page 37 of the official program under the title "An Electric Kymograph, by W. T. Porter."

At the back of each pair of slots, beneath the overhang, is the round head of a metal pin. When either one of the two levers is moved to the right and so is pushed beneath the overhang of a slot, the lever necessarily presses on the top of this pin, forcing it downwards. This pin in turn presses on an adjustable pin in a rocker arm, one of which is shown in Fig. 2. There is a rocker for each slot. When the

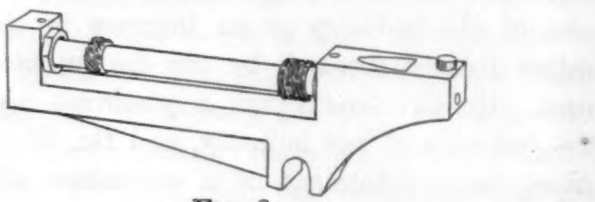

FIG. 2

pin is pressed down the rocker tilts up and its two gears engage with gears upon shafts No. 1 and No. 3. The group of six rockers (left hand) engages with shaft No. 1, and the group of three (right hand) engages with shaft No. 3. The large group (six) is controlled by the left-hand lever, and the small group (three) is controlled by the right-hand lever.

Let us now start the motor and thus cause shaft No. 1 to revolve. By engaging the left-hand lever in any one of its six slots, shaft No. 2 will also revolve. The gears on each of the six rocker arms governed by this lever are so related to the gears of shafts No. 1 and No. 2 that shaft No. 2 may revolve at six

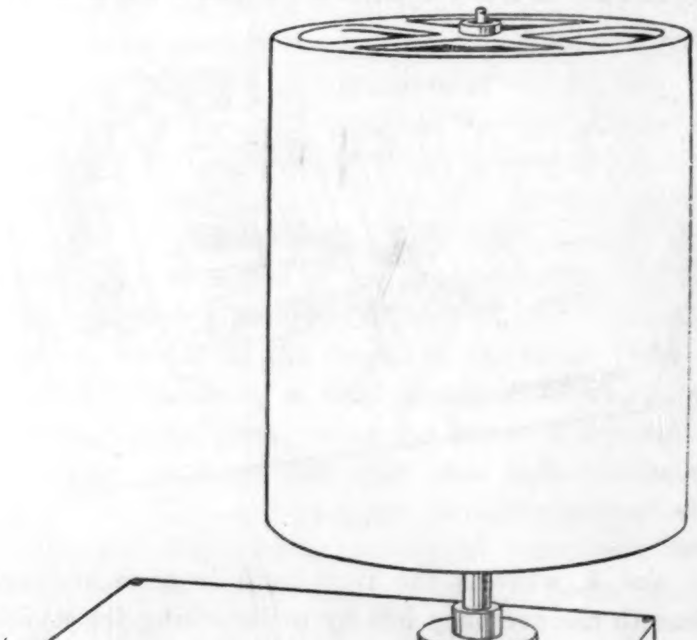

FIG. 3

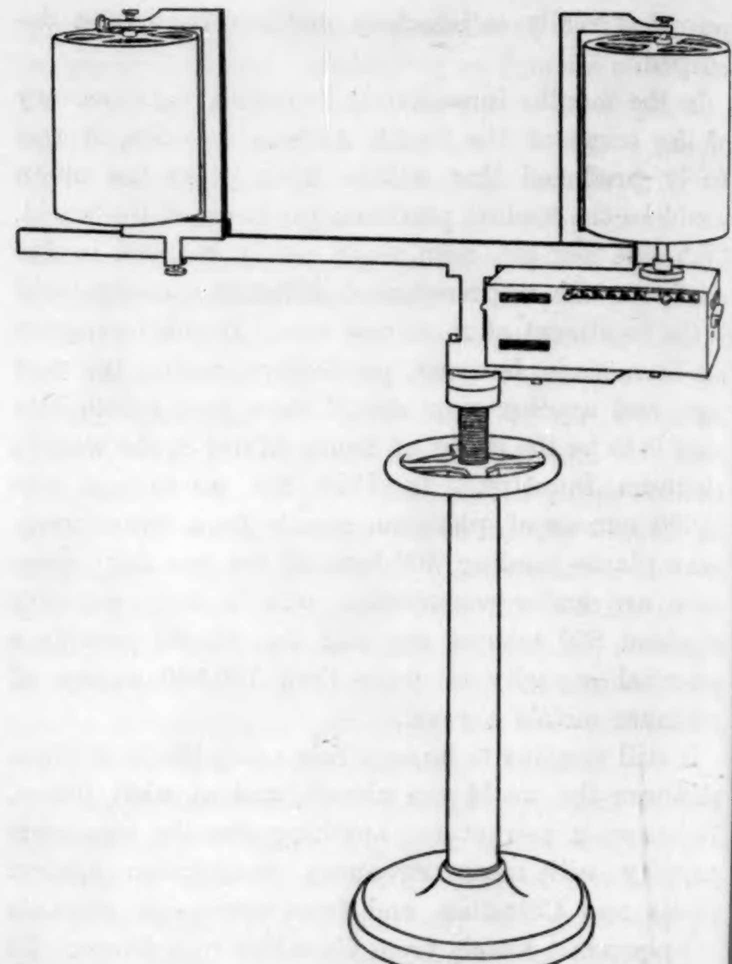

FIG. 4

different speeds, the fastest when the lever is engaged in the left-hand slot.

So we have caused shaft 2 to revolve at any one of six speeds. Meanwhile, the smoked drum is unmoved. It does not turn until the revolutions of shaft No. 2 are carried to shaft No. 3, and thus to shaft No. 4, which last turns the disk on which the drum rests. But the six different speeds of shaft No. 2 are either increased or diminished in their passage to shaft No. 3. When the right-hand lever is engaged in its middle slot (as in Fig. 1), a gear of the middle rocker arm connects with a gear on shaft No. 3 in such a way as to produce a medium set of speeds; when engaged in the left-hand slot, fast speeds will result; and by putting the right-hand lever in its right-hand slot, slow speeds may be had. In each set (fast, medium and slow) there will be six possible changes, one for each position of the left-hand lever. The operator has therefore a choice of eighteen speeds. With a motor giving 1,725 revolutions per minute, these eighteen speeds are as follows:

FAST AND MEDIUM SPEEDS
In millimeters per second

Fast	100	75	50	30	20	10
Medium	10	7.5	5	3	2	1

SLOW SPEEDS
In centimeters per hour

Slow	50	38	25	15	10	5

When the clockwork is used to drive one drum only (Fig. 3), the slowest speed is one revolution in fourteen hours. When two drums are used (Fig. 4), connected by a paper belt about 2.6 meters long, the slowest speed is one revolution in about fifty-two hours. Naturally, the speeds vary with the motor used. The cylinders are 25 cm high and 70 cm in circumference.

As stated above, disengaging the right lever stops the drum, which may then be turned to right or left in order to inspect the tracing or adjust the recording instruments.

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SPECIAL ARTICLES

A POSSIBLE RELATION BETWEEN NATURAL (EARTH) RADIATION AND GENE MUTATIONS[1]

THE discovery three years ago that X-rays and radium produce gene mutations under laboratory conditions has raised the question of how mutations in nature occur.

Muller,[2] Olson and Lewis,[3] Haldane[4] and others have suggested that possibly radiations from the earth, or even cosmic rays, may have played an important rôle in the evolution of species by furnishing heritable variations upon which natural selection may act. It is not improbable that in earlier times radioactive substances were distributed over the earth rather differently than they are to-day and may have been more powerful, as evidenced by the quantity of their end products, helium and lead, now present in the earth. Joly[5] has suggested that cosmic rays may change in intensity and infers that we may be now at the low ebb of a cycle of cosmic radiation. He ties up this decrease of cosmic radiation with the increase of cancer in recent times. Haldane[4] says that "mutants are produced in large quantities by X-rays, and it may be that much of normal mutation is due to the beta and gamma rays from potassium, other radioactive substances and cosmic radiations."

Olson and Lewis[3] were among the first to point out the desirability of testing experimentally the effects of natural radiation upon organisms. According to them, the rays are effective only when absorbed with resulting ionization. Hence the biological effects will be in proportion to the amount of ionization they set up. The above suggestions inspired at least two geneticists to put the matter to an experimental test.

Working independently, Babcock and Collins[6] and the present writers performed almost identical experiments to test this point. Using an electroscope Babcock and Collins discovered a location in a street-car tunnel in San Francisco where the natural ionizing radiation was fully twice as great as the radiation in their laboratory in Berkeley. Accordingly their experiment was designed to compare the rates of occurrence of sex-linked lethal mutations in *Drosophila* in the street-car tunnel and in the laboratory. Three thousand four hundred and eighty-one tests were made in Berkeley, and nine, or 0.26 per cent., produced no male flies and hence showed the occurrence of that many new lethals. Two thousand five hundred tests made in the tunnel gave thirteen, or 0.52 per cent., of lethal mutations. While the difference in rate, 2.5 times the probable error, is not fully significant statistically, it is believed by these authors that it may be fairly so considered. Upon a reanalysis of the data showing the actual experimental variation in rate in the several subgroups in each of the two series it was found that the difference between the average rates for the two locations was increased. This difference was 0.275 ± 0.086.

The present writers, in accepting the implied challenge to experiment contained in Olson and Lewis's paper, considered themselves suitably located for such an undertaking, *i.e.*, near the Ozark caves and lead mines of Missouri. But a long search with the electroscope in the numerous caves and lead mines of this region failed to reveal a location with a sufficient increase of ionization over that of the laboratory, or the middle of a Missouri corn-field, to justify breeding experiments.

Operations were then transferred to Colorado. There in the East Paradox Valley of western Colorado in an abandoned carnotite mine the air was strongly ionized. In addition to the electroscope readings a rough attempt was made to compare the

[1] The expenses of this investigation were met in part by a grant from the committee on the effects of radiation upon living organisms of the National Research Council.

[2] H. J. Muller, "The Problem of Gene Modification," *Verhandlungen des V. Internationalen Kongress für Vererbungswissenschaft*, Berlin, 1928, pp. 234–260.

[3] A. R. Olson and G. N. Lewis, "Natural Radioactivity and the Origin of Species," *Nature*, 121: 673–674, 1928.

[4] J. B. S. Haldane, "The Species Problem in the Light of Genetics," *Nature*, 124: 514–516, 1929.

[5] J. Joly, "Cosmic Rays and Cancer," *Nature*, 124: 579, 1929.

[6] E. B. Babcock and J. L. Collins, "Natural Ionizing Radiation and the Rate of Mutation," *Nature*, 124: 227–228, 1929.

amount of natural ionization in this mine with that of one mg of radium. Radiation in the mine was found to be 0.39 times as intense as that from one mg of radium when the rays were passed through a 0.156-inch lead filter.

Male flies were exposed in this mine for 140 hours and then returned to St. Louis for the breeding tests. The well-known C1B method for detecting lethal mutations in the X-chromosome was employed.

In this technique lethals are revealed in the F_2 cultures. There were 2,860 test cultures, of which seven, or 0.245 ± 0.062 per cent., produced no male flies. In the 1,308 control cultures there was one lethal mutation, or 0.076 ± 0.051 per cent. The difference between tests and controls is 0.169 ± 0.081, a difference 2.09 times its probable error.

While this difference is theoretically not statistically significant it actually may be so. It is highly probable that if the flies could have been exposed for a much longer period than the 140 hours the results would have been more striking. This was impossible in this instance as the time consumed in taking the flies to Colorado and back, together with finding a suitable location there for the test, used up a considerable portion of their life span. The results secured, however, point to much greater success when the experiment can be repeated, hatching the flies, exposing them for several weeks and breeding them for results at the mine. Or a second possibility seems equally promising, namely, from the electroscope readings in the carnotite mine exactly equivalent amounts of radiation can be duplicated in the laboratory and the time of exposure extended accordingly.

These two experiments, one in California and one in Colorado, while falling short of being statistically significant, nevertheless are consistent in that both give an actually higher rate of mutation in flies exposed to natural radiation than in the controls. The least that can be said for the results is that they strengthen definitely the plausibility of the suggestions quoted above to the effect that natural radiation may be responsible for the mutations which are the grist of the natural selection mill with the resulting evolution of new forms.[7]

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[7] We take this opportunity of expressing our appreciation to Dr. R. D. George, state geologist of Colorado, for his many helpful suggestions which led to the finding of what must be one of the most radioactive locations in Colorado; also to Mr. R. S. Blitz, of the Vanadium Corporation, who kindly permitted the tests to be made in the carnotite mine. Mr. Walter D. Claus, of the physics department of Washington University, constructed the electroscope and made the ionization tests.

THE EFFECT OF VARYING THE DURATION OF X-RAY TREATMENT UPON THE FREQUENCY OF MUTATION

IT has been shown repeatedly that X-rays produce variations. We need now to learn more about the nature of the X-ray action in producing these variations, and to obtain further evidence regarding the question whether or not such dilute amounts of radiation as are present in nature might be expected to be producing the mutations found in nature. As a step in that direction, this experiment was begun, in the early part of 1928, at the suggestion and under the supervision of Dr. H. J. Muller, to find the relation between different doses of X-rays and the resultant effect upon the individuals—more definitely, to get the relation of dosage to the frequency of sex-linked lethal mutations produced in *Drosophila melanogaster*, and to analyze the results found.

The different dosages varied only in the length of time of treatment. All other factors were kept as nearly constant as possible. Adult males having the autosomal characteristic brown-eye were collected and kept at 27° C. for at least three days before treatment. The experiment was divided into four series in each of which the flies were divided in about the same proportions among the different dosage groups. All the flies, including the controls, were handled similarly except for the time of treatment. After treatment, these males were mated to virgins containing C1B in one X-chromosome and scute vermilion forked in the other, and after seven days these P_1 flies were discarded. The F_1 bar-eyed (C1B-containing) females were mated to their brothers. Normally half the F_2 males die because of the lethal in the C1B combination. Now if a lethal had arisen by treatment of the X-chromosome of the P_1 male, there would be no F_2 males appearing. However, it is necessary to check these apparent lethals in the F_2 cultures by further breeding in order to be certain that the no-male result was due to a lethal and not to other conditions. For that purpose, the non-bar-eyed F_2 females in the apparently lethal cultures were mated to scute vermilion forked males. Only the cultures showing a lethal in these F_3 results were recorded under column three in the table given.

The results are tabulated briefly in the following table in which "dosage" refers to duration of treatment: t_1 lasted three and one half minutes; t_2, double that time, etc. As previously indicated, the total number of lethals is based on the F_3 results from the matings of the non-bar females by sc v f males. The "per cent. of observed lethals due to treatment" is found by subtracting the control lethal per cent. $(0.24 \pm .051)$ from the per cent. found for each dosage. The "ideal per cent." is calculated from the

Dosage	Number fertile F_2 cultures	Total number lethals	Per cent. observed lethals due to treatment	Ideal per cent. lethals
t_{16}	435	70 ± 5.19	15.85 ± 1.19	$18.07 \pm .7$
t_8	618	61 ± 4.57	$9.63 \pm .74$	$9.49 \pm .4$
t_4	1144	55 ± 4.89	$4.56 \pm .428$	$4.86 \pm .2$
t_2	2231	72 ± 5.63	$2.99 \pm .256$	$2.46 \pm .1$
t_1	4016	57 ± 5.04	$1.18 \pm .135$	$1.24 \pm .05$
Control	4033	10 ± 2.13		
Total	12477			

X-ray dosage: 50 KV; 10 MA; 13½ cm distance; 1 mm Al screen; time varies as indicated; t_1 approximately 285 r units.

equation based upon the law of probability: $\log q = kt$, in which q is the proportion of escaping (non-lethal) cells, t is the time of treatment and k is a constant. From this equation the mean value of k is found, and the ideal values for all the doses used are calculated from that.

The results show a significant increase in the per cent. of lethals each time the dosage is doubled, and with each dosage, the per cent. of corrected observed lethals approaches closely the ideal per cent., that is, the per cent. that would be expected based on the results as a whole, if the above equation held true. That there is a direct proportionality between the per cent. of lethals and the length of time of treatment may be seen more readily by a comparison of the t_1 values calculated from the results for each of the given doses. These values are calculated, based on the proportion of escaping cells, by using the formula previously given. The mean t_1 value is found to be $1.24 \pm .05$; t_1 values calculated from the results for each of the given doses are found to be: t_1 based on t_1 result $= 1.18 \pm .13$; t_1 based on t_2 result $= 1.506 \pm .13$; t_1 based on t_4 result $= 1.162 \pm .11$; t_1 based on t_8 result $= 1.26 \pm .10$; t_1 based on t_{16} result $= 1.074 \pm .09$.

A comparison of these values with the mean t_1 figure shows that the values based on the t_1, t_4 and t_8 doses approach closely to this mean; those of the t_2 and t_{16} vary more, the former being larger and the latter smaller. However, in each case, the difference between the observed and ideal values falls within twice its own probable error, an event to be expected once in about 5.5 trials, on the average, and hence not surprising in two of our five batches.

In the analysis of the F_3 cultures one finds some in which there is a significant decrease in crossing-over. Some of these are lethal in effect and so were recorded as lethals in the table; however, there was such a lack of crossing-over that it was impossible from our recorded results to locate the lethal or to say just what kind of lethal it was. For the present these are classed as "chromosome abnormalities" (CA). The two higher doses have a much greater per cent. of the CA lethals than do the three lower doses. We found, corrected, 4.88 per cent. of such CA's in the t_{16}, 2.204 in t_8, 0.377 in t_4, 0.403 in t_2, 0.075 in t_1 and none in the control. Calculating the t_1 value from these, the average for the two higher doses is $.292 \pm .0465$, and for the three lower doses, $.124 \pm .029$. There is, then, a difference of $.168 \pm .037$. This difference is 4.5 times its probable error, and this is apparently significant. However, it may be that some of these CA's are due to double lethal point-mutations or to lethal point-mutations combined with semi-lethal or poor-viability genes. Tests are now being made, to be reported later, by which to analyze them. The effect of dosage on the frequency of CA's can not be conclusively given till these tests are completed.

Regardless of the results for the CA's, it is important to note that the total number of lethals is directly proportional to the dosage used when the only factor varied is the duration of treatment. There is no indication of a threshold dosage below which mutations would not be produced. In so far as this work goes, it therefore indicates that the small amounts of radiation in nature may cause some or all of the natural mutations. Since the suggestion of this possibility and of the method of testing it by measurements of mutation frequency in the presence of different dosages of radiation was made by Muller,[1] Olson and Lewis[2] reported calculations indicating that in tobacco the frequency of "variations" under natural conditions bore about the same relation to their frequency following X-ray treatment as did the amount of ionization in nature to that caused by the X-raying. It is, however, uncertain whether or not gene-mutations were predominantly involved in this work and whether the types of variation in the two cases were comparable genetically. A few months later Hanson and Heys[3] obtained results in favor of the interpretation

[1] H. J. Muller, "Artificial Transmutation of the Gene," SCIENCE, 66, 84–87, 1927; "The Problem of Genic Modification," *Verhand. d. V. Int. Kong. f. Vererb.*, Berlin, 234–260, 1927; "The Production of Mutations by X-Rays," *Proc. Nat. Acad. Sci.*, 14: 714–726, 1928.

[2] A. R. Olson and G. N. Lewis, "Does Natural Ionizing Radiation Control Rate of Mutation?" *Nature*, 121: 673–674, 1928.

[3] F. B. Hanson, "An Analysis of the Effects of Different Rays of Radium in Producing Lethal Mutations in Drosophila" (Abstr.), *Anat. Rec.*, 41: 99–100, 1929; F. B. Hanson and F. M. Heys, "An Analysis of the Effects of Different Rays of Radium in Producing Lethal Mutations in Drosophila," *Am. Nat.*, 63: 201–213, 1929.

that natural radiation caused natural mutations, since their data on beta and gamma rays of radium, like the independent work on X-rays reported above, showed a proportionate relationship between dosage and mutation frequency. Stadler[4] on the basis of preliminary experiments on barley has tentatively reported an apparently similar relation between X-ray dosage and mutational effect. Still more recently Babcock and Collins[5] have reported preliminary results that point in the same direction, in that they find a difference in mutation frequency between two series of flies subjected to differing amounts of natural radiation which is 2.5 times its own probable error. The chance of occurrence of such an outcome if there were no real effect is 1 to 10. In their work, owing to the small numbers of mutations necessarily obtained with such dilute radiation, there can as yet be no question of showing a proportionality between mutation frequency and radiation. However, the concurrence of the evidence from all the above sources is noteworthy.

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OVARIAN CHANGES DURING PREGNANCY IN THE RAT

IN an earlier issue of SCIENCE, Nelson[1] reports the recurrence of oestrus cycles four days in length with copulation taking place at three of the oestrus periods in a pregnant white rat. Two instances of copulation during pregnancy had been reported by Long and Evans,[2] though no evidences of the normal oestrus cycle were noted. In the later report no attempt was made to determine the occurrence of ovulation.

In a preliminary communication[3] we have noted the occurrence of ovogenesis during adult life in the mammalia as a rhythmical production which coincides with the rhythm of the oestrus cycle. In each oestrus cycle a new crop of follicles is formed and a few grow to maturity. At each oestrus period ovulation takes place, and all the other follicles degenerate within a short time thereafter. We have found that this cycle of ovogenesis in the rat is not interrupted by pregnancy, but continues throughout with the usual four or five-day periods. At the end of each period ripe follicles are present with many smaller follicles showing all stages of degeneration. With the beginning of the new cycle at the fifth, the tenth, the fourteenth and the eighteenth days, or thereabouts, newly formed corpora are found, some of which contain a segmenting ovum. These corpora are seldom more than a third of the size of the corpora of pregnancy, and are often much less than that. The number of large follicles produced at each cycle varied from two to twelve in the seventy-six rats that were examined at all stages of pregnancy from the third day to full term. The number of small corpora showed about the same range of variation though occasionally small follicles were luteinized.

No changes in the uterus corresponding to those of the non-pregnant oestrus cycle were observed. The living animals were not examined by means of the vaginal smear method. This has been done in hundreds of pregnant rats in our laboratory, however, without detecting the typical oestrus changes, though it has been noted for many years that at day five the smear loses its typical appearance, resembling a prooestrus smear, especially in the reduction in leucocytes, though it is not followed by cornified cells. In their studies of the uterus of pregnant rats, Long and Evans[2] found no evidence of cyclical changes. No eggs were found in the tubes, though comparatively few of the corpora showed a retained egg.

Twenty-three of these rats were tested for oestrus behavior by being placed in a cage with an active male on the fifth day. No evidences of oestrus were observed and copulation did not take place in a single instance, as shown by the absence of plug and sperm when examined on the following day.

There is no evidence in these animals that the corpora lutea of pregnancy have any effect on the production of a normal number of follicles and their maturation. The evidence also shows that the cervical stimulation of copulation at the beginning of pregnancy did not result in the delayed production of mature follicles, as these were present at the fifth day in eight rats. It also indicates that the presence of fairly large follicles in the ovary is not sufficient to produce oestrus changes in the uterus or oestrus behavior in the animal, although corpora lutea are actually developed from such follicles. Cyclical changes thus occur in the ovary during pregnancy, a condition which has hitherto been supposed to suspend those changes.

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[4] L. J. Stadler, "The Rate of Induced Mutations in Relation to Dormancy, Temperature and Dosage" (Abstr.), *Anat. Rec.*, 41: 97, 1928.

[5] E. B. Babcock and J. L. Collins, "Does Natural Ionizing Radiation Control Rate of Mutation?" *Proc. Nat. Acad. Sci.*, 15: 623–628, 1929.

[1] W. O. Nelson: "Oestrus during Pregnancy," SCIENCE, 70: 453, November 8, 1929.

[2] J. A. Long and H. M. Evans: "The Oestrus Cycle in the Rat and Its Associated Phenomena," Mem. Univ. Calif., Vol. 6, 1922.

[3] O. Swezy and H. M. Evans: "Ovogenesis in the Mammalia," *Proc. Exp. Biol. and Med.*, Vol. 27, 1929.